

# LAMONT-DOHERTY EARTH OBSERVATORY OF COLUMBIA UNIVERSITY



*Twelve Perspectives on the First Fifty Years*  
1949-1999





## TALES OF THE KALAH

BOOK SIGNING PARTY

with David P. Johnson, author of  
Fieldwork: A geologist's memoir of  
the Kalahari

Fieldwork:  
A geologist's memoir of  
the Kalahari

with David P. Johnson, University of  
California, Berkeley

## NOAH'S FLOOD

THE NEW SCIENTIFIC DISCOVERIES  
ABOUT THE EVENT THAT CHANGED HISTORY  
WILLIAM KERN  
ZEWATER PUTMAN



ENINO





The ship about 1940-41

"Once we survey a place, we know what to come back and look for.  
It's not too hard, but with some intelligence and a little luck, it's a lot more. We can't look at enough  
places until that we won't know what the most interesting places are until we've seen all of them.  
It may sound crazy to want to survey everything, but that's what we do want.  
So we can say of the ocean, 'It is flat and so.' — Maurice Ewing









*Doc Ewing aboard R/V VEMA*

*"Once we survey a place, we know what to come back and look for.  
It sounds hit-and-miss, but with some intelligence and a little luck, it's a lot more! We can't look at enough.  
It's my view that we won't know where the most interesting places are until we've seen all of them!  
It may sound egotistic to want to survey everything, but that's what we do want.  
So we can say of the ocean, 'It is thus and so'." ~ Maurice Ewing*







LAMONT-DOHERTY EARTH OBSERVATORY:

TWELVE PERSPECTIVES  
ON THE FIRST FIFTY YEARS  
1949-1999

*Edited by Laurence Lippsett*



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## FOREWORD

Presented here are twelve perspectives of Lamont-Doherty Earth Observatory's first fifty years. Each chapter was written by a Lamont-Doherty scientist. The volume was edited by Laurence Lippsett, who served as a science writer at Lamont-Doherty from 1992-1996. It was designed by Janice Aitchison, manager of creative design at the Observatory.

We are indebted to these two professionals for their fine work and to the authors of each chapter who devoted a great deal of thought and time to their contributions.

In addition, we would like to acknowledge the work of Dave Lawrence, who helped research Marie Tharp's chapter, and Patty Catanzaro, who compiled the historical list of geochemistry personnel and assisted us with photo research. And finally, a major thank you is due Julie Lipkin and Stanley Yates, who did a meticulous job in proofreading the entire volume.

We hope that you will enjoy reading this as much as we enjoyed producing it.



# FOREWORD

The purpose of this book is to provide a comprehensive survey of the history of the United States from the time of the first European settlement to the present. The book is divided into two main parts: the first part covers the period from 1492 to 1776, and the second part covers the period from 1776 to the present. The first part is divided into three sections: the first section covers the period from 1492 to 1600, the second section covers the period from 1600 to 1700, and the third section covers the period from 1700 to 1776. The second part is divided into two sections: the first section covers the period from 1776 to 1800, and the second section covers the period from 1800 to the present.

The book is written in a clear and concise style, and it is suitable for use as a textbook in a college or university. The book is also suitable for use as a reference work for anyone interested in the history of the United States.



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# INTRODUCTION

by Laurence Lippsett

It's hard to walk out of the Sistine Chapel without taking along a lasting impression. I felt similarly when I first visited the Core Laboratory at the Lamont-Doherty Earth Observatory.

I was a graduate journalism student at Columbia University, with an inkling that I could find some poetry in science and write about it. Lamont was a research institute of Columbia in Palisades, New York, so it provided our professor with a convenient field trip to let loose his students to exercise their pubescent reporting skills.

Unlike the Sistine Chapel, there was nothing about the core lab that was remotely aesthetically pleasing. It was a vast warehouse with cement floors and mammoth, monotonous banks of gray, industrial shelving that were filled floor to ceiling with nondescript tubes containing, essentially, mud. We were told that the tubes—thousands of them—contained specimens from the bottom of the ocean, from every ocean in the world.

A feeling of solemnity enveloped us. It felt like we were standing before a pyramid. The sheer enormity of it hushed us—not the enormity of the room, but of the vision and determination and effort that caused the core lab to exist.

I could imagine decades of voyages, millions of nautical miles, raging storms, ice-covered seas, humbling waves, straining machinery, straining muscles, hundreds of problems ingeniously solved, thousands of sunsets away from home and family. Whoever did this must have been as relentless as a horde of locusts mowing down a cornfield. But unlike

locusts, *their* numbers were far fewer and their target much more vast and inaccessible.

It left an impression.

A decade later, I was lucky enough to be hired by Columbia to report on its scientific research and Lamont was part of the beat I covered. The core lab, I soon learned, was merely the tip of the iceberg. Lamont is a singularly extraordinary place—one that fundamentally changed our understanding of the planet we live on.

In 1935, when Maurice (pronounced “Morris”) Ewing, Lamont's founder, began his research on the Earth, the planet was about as well-understood as the human body was in the Middle Ages. The oceans, the seafloor and what lay beneath it were almost entirely unknown. The forces that created oceans, continents, mountains, volcanoes, earthquakes and ice ages could only be guessed at.

In those days, the disciplines of physics and geology existed, but they had never mixed. Trained as a physicist, Ewing had launched a few efforts to use sound waves, created with explosives, to examine subsurface geological rock layers on land. Then the Geological Society of America gave him a few thousand dollars to try those same techniques to investigate the seafloor.

“If they had asked me to put seismic equipment on the moon instead of the bottom of the ocean I'd have agreed, I was so desperate for a chance to do research,” Ewing told his biographer William Wertenbaker in his book *The Floor of the Sea*. (In fact, Ewing and other colleagues at Lamont did



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put such equipment aboard Apollo flights and onto the moon decades later.)

"After the first work in 1935," Ewing wrote in 1955, "I realized that I had found the kind of work which I would like to do the rest of my life." He saw vast potential in exploring the oceans and sought "small annual grants" from several oil companies "to support a modest program of research."

"This proposal received no support whatever, and I was told that work out in the ocean could not possibly be of interest to the shareholder and could not rightfully receive one nickel of the shareholders' money," Ewing said.

But Ewing's thirst for knowledge was absolutely unquenchable—and absolutely contagious. With a few modest grants from the GSA and the National Geographic Society and a fellowship from the Guggenheim Foundation, Ewing and a few students began to attempt experiments that had never been conceived before, let alone done.

10 | "We were physicists and engineers, using our wits and flying by the seat of our pants to bring these sciences to bear on the study of the Earth, working out the methods as we went along," said J. Lamar Worzel, who started as one of Ewing's students in 1937, became associate director of Lamont, and worked with Ewing until Ewing died in 1974.

Commercial geophysical instruments didn't exist, so Ewing and his students designed and built them themselves. It was the Depression, and so the group begged, borrowed and bartered to get whatever they needed. They pinched pennies and jury-rigged state-of-the-art equipment, using fruit salad cans, drinking glasses from diners, pocket watches and electric motors from toy trains. They weren't above accepting castoffs and converting them into breakthroughs, such as the surplus Navy artillery shell from the Bethlehem Steel Co. that they fashioned into a pressure-testing device. They slept in fields to save money and washed their photographic records in bathtubs. They had only two precious weeks of ship time per year to test their equipment and conduct their experiments, as guests on other people's research vessels. They rarely wasted a moment or missed an opportunity. Nothing deterred them from their mission.

World War II brought new urgency to research on the oceans and a surge of Navy funding. Ewing and colleagues applied their nascent techniques using explosives and sound waves in the ocean to the water column itself. They revealed for the first time the intricacies of how sound is transmitted through the oceans. The Navy immediately put that knowledge to good use in anti-submarine and mining operations, and Ewing's team went on to design new apparatus that saved ships, submarines and lives.

The field of geophysics had attained a beachhead and Ewing and a handful of students came to Columbia to establish an academic base for their young science. On the Morningside Campus, they first holed up in hastily refurbished rooms in Schermerhorn Hall, where vibrations from subways confounded their budding land-based seismic experiments. And, of course, they were still shipless.

Wartime research, which had spawned sonar advances, the radar and the atomic bomb, had demonstrated the power of science and ushered in a new era of unprecedented funding for basic research. Nevertheless, Ewing and his crew, like most people who lived through the Depression, never took anything for granted and gladly took anything anyone would give them.

In late December of 1948, Florence Lamont, widow of the financier Thomas Lamont, donated the family estate in Palisades to Columbia. Not long after, Ewing and his merry band were installing seismometers in the estate's root cellar and abandoned indoor swimming pool. These seismometers, designed by Ewing and Frank Press, were prototypes of instruments that would soon be installed around the globe to create the first worldwide network to record seismic waves traveling through the Earth and along its circumference. Bedrooms were converted into offices. Ewing and company moved into cottages formerly used by the estate's chauffeur and groundskeeper, creating a village of scientific homesteaders. The embryonic sediment core collection was spread beneath splendid chandeliers in a former dining room. The kitchen, with its gas line, running water and drains, naturally suited Lamont's pioneering

geochemistry program. Lamont's young geochemists, led by J. Laurence Kulp, were poised to unleash modern post-war chemistry techniques and equipment on the study of geology—much the way Ewing was applying physics to it.

Established on land, the newly named Lamont Geological Observatory fulfilled its next desperate need: its own ship. In 1953, Lamont grabbed another luxurious castoff, a down-on-its-luck yacht originally built for E.F. Hutton called the *Vema*. To get her, Ewing and Worzel tracked down Columbia's treasurer, Joseph Campbell, on a golf course. Campbell put up \$80,000 of his own money so that Lamont could buy the *Vema* before an option expired at midnight. They didn't even have time to consult Columbia's trustees.

Ewing hired Angelo Ludas, a veteran of the Manhattan Project at Columbia, to establish and run a machine shop to translate into metal and wires all the instrumental visions of Lamont's scientists. Arnold Finck came aboard to handle the observatory's growing administrative tasks (for which Ewing was quite unsuited). Later, Henry Kohler was hired to captain the *Vema*. These were "can do" guys, for whom obstacles and problems made life interesting, but never seriously deterred them from accomplishing whatever had to be done to complete a scientific mission. They were Lamont mainstays who all devoted the rest of their careers to the observatory.

Soon the pieces and people were in place. In April of 1955, Ewing wrote: "I believe that we have built up here a unique team of scientists, unique in the diversity of techniques which it can bear on problems and in the fundamental importance of the problems in which the group is interested. . . . It is certainly a privilege which few men have had to see this group now up in twenty years from nothing to its present state. In my opinion we have really just now started.

"I believe that this integrated group of scientists, this group of facilities, which includes the ship, the chemical laboratory, the collection of sediment cores, and the great seismograph station, constitutes a facility comparable with the greatest cyclotron or the greatest telescope, and it is unique. It is as though there was just one cyclotron in the

world and we had control of it, or just one big telescope in the world, and we had control of it."

The new institution was called an observatory for good reason, and the mission was to venture forth and make observations. If Lamont had a litany it would go something like this: *Data will reveal knowledge. The more data, the more knowledge. Acquire, or build, or invent whatever you need to collect as much as you can, as efficiently as you can.*

Ewing's drive set the tone and the pace. Nothing was more important or interesting to Ewing than his pursuit of knowledge. The light burned late in his office every night, seven days a week. Sleep was a waste of time to him, and he waged a personal daily battle against it. He often scribbled furiously in little books just to keep himself awake. Once he took a vacation but cut it short after two days. Being on vacation unnerved him so much, he had to return to Lamont.

"Ewing was a hard man to work for and with, pushing you to the limit of your ability," Worzel said. "But what kept us all from chalking it off and going elsewhere to work with someone more reasonable was his brilliance."

Under her bowsprit, *Vema* had a huge, determined-looking eagle, which well represented Lamont's indomitable spirit. (Today it hangs in the front hall of Lamont's Geosciences Building.) Never forgetting the old days when ship time was precious, Ewing and company stocked that ship with every conceivable tool to measure any conceivable piece of data, making entirely new devices all along the way and continually solving problems large and small. For example, Bernie Luskin invented the precision depth recorder, which gathered continuous profiles of the seafloor in unprecedented detail. On an early rough voyage, water leaked into a room and dangerously onto a floor where the PDR's batteries lay. Luskin and Worzel spent an exhausting night holding the high-tech PDR aloft with one hand, and with the other, bailing water with a low-tech dustpan—all the while making plans to build a rack for the PDR as soon as possible.)

The *Vema* was also noted for its prodigious rolling, so a specially designed stabilization system was built for her,



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with a tank filled with some three thousand gallons of water. Once in Desperation Bay near Antarctica, the *Vema* began rolling madly. The tank had frozen. (In the Lamont spirit of getting maximum use out of everything, Worzel joked that Captain Kohler could solve the problem by filling the tank with rum. With equal facetiousness, Kohler parried that such a reservoir in such close proximity to a hard-working crew so long at sea might be counterproductive.)

"Ewing expected scientists to use the ship every minute of every day," said Dennis Hayes, a Lamont geophysicist who first joined the observatory in 1961. "While scientists at other institutions typically collected data solely for their own individual research, Lamont scientists had standing orders directly from Ewing to collect as much data and as many different kinds of data as possible—regardless of whether the scientists aboard had any personal interest in them."

The *Vema*, and later Lamont's second ship, the *Robert D. Conrad*, ceaselessly circled the oceans like two orbiting moons. They routinely and continually collected precision depth recordings of the seafloor, seismic reflections of the layers below the seafloor, gravity and magnetic measurements, probes of the heat flow through seafloor crust, seafloor sediment cores, ocean bottom photographs and more.

The work wasn't without danger. In 1954, while trying to secure fuel drums that had broken free on deck in heavy seas off Cape Hatteras, Ewing, his brother John, First Mate Charles Wilkie and Second Mate Mike Brown were swept overboard by a huge wave. Somehow, the ship's captain, Frederick MacMurray (the former captain of Woods Hole Oceanographic Institution's research ship *Atlantis*, whom Ewing had briefly lured out of retirement) managed to turn the ship around in rough seas and pick up Brown. A lookout happened to notice John Ewing and threw him a line. Forty-five minutes later, Maurice Ewing was sighted. He could not grab a rescue rope because he had been hit in the neck when he was washed overboard and his left side was paralyzed. The ship drifted toward him and on a severe roll, when Ewing was alongside and the ship's rail touched the sea surface, a crewman reached under Ewing's

armpits and swept him aboard. First Mate Wilkie was never found. After a week, Ewing mostly recovered, though he walked with a limp from then on. Of course, the incident in no way curtailed his time at sea nor slowed him down.

In 1961, chief scientist John Hennion was killed in an explosives accident, the only such fatality through all the years of using explosives for their work. The accident precipitated Lamont scientists and technicians to find another way to conduct these experiments. They developed airgun technology that is still used today to create sound waves for seagoing seismic research.

By the middle 1960s, all those years of relentless data-gathering added up. Each piece of data from one part in the ocean was like a dot in a pointillist painting or a tiny piece in the massive, moving jigsaw puzzle that is our planet. Or put another way, they were like individual frames of an ongoing (though slow-moving) video that could be spliced together and played backward through millions of years to reveal the full story of the Earth.

The seafloor, previously believed to be barren, static and featureless, was wondrously unveiled. Analyzing the new wealth of bathymetric data, Bruce Heezen, Marie Tharp and Ewing discovered the largest geological feature on Earth—the globe-encircling mid-ocean ridges, where magma erupted from the mantle to create new seafloor. Seismologist Lynn Sykes used seismic data to uncover the telltale movements of earthquake-prone transform faults near mid-ocean ridges. Seismologists Jack Oliver and Bryan Isaacks led efforts to discover subduction zones, where old seafloor plummeted back into the mantle. Walter Pitman and Jim Heirtzler analyzed magnetic data (which were dutifully collected even though no one at the time had any real idea how they might be used) and discovered a mirror-image pattern of magnetic changes in the rocks on either side of a mid-ocean ridge. They pinned an image of this magnetic data—the so-called "magic profile"—to the door of a lab. Few papers have had such an instantaneous transforming impact since Martin Luther nailed his ninety-fives theses on a church door in Saxony in 1517.

The magic profile jolted even Ewing, who, like the majority of scientists of the day, hadn't believed that the seafloor spread out from the mid-ocean ridges to create new ocean basins and to split continents and move them around on the face of the Earth. Nevertheless, Ewing didn't dissuade his disciples from following wherever the data led them and pursuing the truth.

In just about two decades, Lamont had dissected the planet and reassembled it in a revolutionary new way. The mass of accumulated evidence was overwhelming and convincing. The theory of plate tectonics was confirmed, providing a new foundation that explained a wide range of geological phenomena and features on the Earth.

In much the same vein, Lamont continued on through two name changes. In 1969, the observatory added "Doherty" to its name in recognition of a generous gift from the Henry L. and Grace Doherty Charitable Foundation. In 1993, it was renamed the Lamont-Doherty Earth Observatory to embrace the ever-expanding scope of its research.

Revolutionary discoveries continued, especially by the geochemists, who hadn't had the head start of the geophysicists. Creating and advancing their science nearly simultaneously, they unraveled the circulation of the oceans, identified environmental problems (including contamination from radioactive nuclear weapons tests and from phosphates in detergents), and began to piece together the workings of Earth's ever-changing and delicately balanced climate system. In the 1970s Lamont paleomagnetists and geochemists combined to help show that Earth's climate was about as dynamic as its seafloor. They demonstrated how ice ages waxed and waned, paced by changes in Earth's orbit.

The list of scientific accomplishment goes on. In the 1980s, oceanographers Mark Cane and Steve Zebiak created the first computer model to predict El Niño, setting the stage for breakthroughs in short-term climate prediction. In the 1990s, seismologists Paul Richards and Xiaodong Song took advantage of Lamont's huge archive of seismic data to make their remarkable discovery that Earth's solid inner core is rotating independently and slightly faster than the rest of the planet.

Once, passing through the core lab, I saw two graduate students Jerry McManus and Sean Higgins, sitting on either side of a table with a sediment core between them. Each was meticulously extracting samples from either end of the core. "Jerry's up there in the Eemian (a so-called interglacial period some 125,000 to 115,000 years ago) when it was nice and warm all the time," said Higgins. "I'm down here when all hell was breaking loose." He was working about two feet and tens of thousands of years beyond McManus, in mud deposited on the seafloor during Earth's most recent ice age.

The scene reminded me that as much as anything, Lamont has always been a unique, close-knit village of energetic, insatiably curious people who combine the cleverness of detectives and the dedication of soldiers—along with a vision that can encompass the entire planet throughout time, and all the possibilities therein. That village has grown to nearly 500 scientists, students, technicians and staff.

There are so many more stories about Lamont than can be told in this volume and more remarkable people than can be mentioned. On the occasion of the observatory's fiftieth anniversary, we asked several Lamonters, in various fields and from various generations, to reminisce about their experiences here. This is not the definitive scientific, or historical, treatise on Lamont. We simply hoped to capture, in the words of a few of the many people who played a part, the essence and spirit of this place.

It's a powerful, almost palpable spirit, and that's what I felt when I first visited the core lab in 1980. But even more sophisticated people feel it, too. I remember another time in the core lab, several years later, when I encountered a noted scientist from another institution.

"This is hallowed ground," he said. It wasn't the first adjective I expected from a scientist, but by then, I knew exactly what he meant.

*To Fred Knubel,  
who was there to tell Lamont's story for many years,  
and who was always there for me.*

— L.L.







*"If you don't have the proper equipment and there's no hope of getting it for a month, but you HAVE to have something, you find a way . . . and that's pretty much how the Lamont-Doherty Earth Observatory was built."*





# ROOTS AND RHYTHMS

## *The Gestation and Birth of Lamont*

by J. Lamar Worzel

It all began for me late one Friday evening in the fall of 1937, when Allyn Vine came to my room and asked if I wanted to go the next morning to dig holes in New Jersey. Vine and I rented rooms in the same boarding house near Lehigh University, where I was a sophomore and he was a beginning graduate student. Vine and another student, Norman Webster, were going to join professors Maurice Ewing of Lehigh and George Woollard of Princeton to take profiles of the subsurface geology of New Jersey, using seismic waves generated by explosives. The crew was shorthanded and Vine asked if I'd like to help dig holes to place the explosives in. I did some soul-searching because I would have to cut a Saturday-morning class. But I opted to go along—a decision, I guess, that settled the rest of my professional career.

On that weekend, we left Bethlehem, Pennsylvania, about five a.m. and drove to Princeton in Floozey Belle, Ewing's 1934 Ford. Floozey Belle was a product of the Depression. She had two front bucket seats, a back door and a removable back seat so that she could double as a delivery truck. That Saturday, the back seat was removed and three of us sat in front, with a fourth person sharing the space in back with an oscillograph, amplifiers, cables and geophones. Hand augers, extension pipes and a reel of conductor wire were strapped on the outside.

At Princeton, Woollard joined us with his Model A Ford filled with cases of dynamite and we drove about forty miles toward Barnegat Bay near the Jersey coast. We parked Floozey Belle near a utility pole and placed geophones in

front and in back of her, 200 feet apart along a secondary road. These would record seismic waves traveling from our explosion site through intervening rock layers. Then we strung conductor wire along the road to the sites where we shot off the explosives, which varied from 100 feet to eight miles away. With hand augers we dug shot holes, six inches in diameter and ten to forty feet deep, along the road as near as we could to fence lines. If we were accosted by a landholder, we would say we weren't on his land but on the road right-of-way. To government objections, we would aver that we weren't on the road right-of-way but on private land.

When the holes were loaded with explosives we would telephone Ewing at Floozey Belle that we were ready. When he had warmed up the amplifiers and the geophones were quiet, Ewing would tell us to shoot. He would hand-crank photo paper through the oscillograph until the sounds had all arrived at the geophones. When the dynamite exploded, it was instantaneously transmitted along our telephone wire to the oscillograph. By timing the arrival of the various sound waves at the geophones, we could calculate how fast the sound waves traveled through the various, nearly horizontal layers beneath us. Knowing that, we could calculate the thickness of each layer.

After a recording was completed, Ewing would put the photo paper in a portable light-tight box and put the box in "Minnie's drawers," a black mantle-covered box that contained cans of developer, water and fixing agent. By winding the paper back and forth in each can appropriately,



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he would develop the record, examine it and decide where to shoot next. We did that the whole weekend and didn't return to Bethlehem until after dark on Sunday.

The whole experience was full of adventure, a young man's delight. After that first weekend, I became a regular on these weekend seismic expeditions, which continued through the fall and resumed in the spring of 1938. When my classes ended at noon Friday, I'd go to the lab and join whoever was working to get the equipment ready and loaded on Floozey Belle. We'd get some sleep and leave for New Jersey before dawn to have as many daylight hours as possible. As our locations moved closer to the coast, we'd have to leave earlier. We would work until dark, eating sandwiches and drinking milk at noon without interrupting the work. After dark we'd get some supper, find a nearby rooming house, wash the records thoroughly in a bathtub, hang them all over the bathroom to dry, read the records carefully, plot the data on graph paper, calculate the velocities and thicknesses of the subsurface layers, and make plans for the next day. We'd go to bed at midnight, waking up at five a.m. to get to work at daylight. We'd drive back to Bethlehem Sunday in the dark and unload the equipment, because Ewing had to use the car for his family during the week. I'd often get back to my room at two a.m. and would have to get up four or five hours later to get to my first class. During the week, in addition to our classwork, we would often have to make repairs to our gear. I got by on little sleep.

Ewing was my physics professor at Lehigh, but when I joined his merry band I soon called him "Doc," as everyone else did. And everyone called me "Joe," though my real name is John Lamar Worzel and my family called me "Lamar." In 1937 there was a popular song with the refrain: "What do you know Joe? We don't know nothing." For awhile, everyone at Lehigh greeted guys with that refrain and called everyone else "Joe." The fad faded, but perhaps because "Lamar" was more unusual, people kept calling me "Joe," and that's what people called me my whole working life. Many people I've worked with for decades still don't know my real name is Lamar.

Early on in our Jersey work, our connection to Floozey Belle failed and we found that our line had been cut and a long piece taken away, which precipitated our decision to use radio connections instead of phone lines. Bob McCurdy, an amateur radio nut, was recruited to join our group and became a regular. The State Geologist of New Jersey, who was financing our work, would occasionally visit us in the field and help out. He never inquired about the legality of using dynamite or unlicensed radios, and we didn't burden him with such details.

Early one morning, crossing the central square of Silverton, New Jersey, in my Ford coupe, a bread truck ran into my rear fender. I had to fend off a local body shop worker who insisted on opening the rumble seat to assess the damage. It was filled with dynamite that I was transporting—not exactly legally.

Sometimes we'd have to stop traffic because the noise would bother our instruments and because we didn't want any explosive debris hitting cars. One Saturday afternoon, I recall, Woollard stopped a car coming along the road, saying with his charming Southern accent, "Sorry, sir, we're about to do some work down here. You'll have to wait a few minutes." In a rush, the driver protested, and Woollard reached in and took his keys. When we got our shot off, Woollard said, "Thank you, sir" and returned the keys.

Nearly all the instruments we used were made by Ewing and his early graduate students, Albert Crary and H. M. Rutherford. Commercially available geophysical instruments didn't exist at the time. Geophysics as a science didn't really exist. We were physicists and engineers, using our wits and flying by the seat of our pants to bring these sciences to bear on the study of the Earth, working out the methods as we went along. Most physicists and geologists thought our nascent exploratory efforts were bastardizations of each of their sciences. Ewing himself never took a geology course in his life, though he grew up in Texas and was quite familiar with emerging seismic techniques that oil companies were using to search for oil.

In 1936 Major William Bowie of the Coast and Geodetic Survey and Richard Field of Princeton had urged the

Geological Society of America (GSA) to explore whether seismic measurements being pioneered on land might also be used to study the geology beneath the seafloor. They naturally approached Ewing, who was the only academic scientist doing anything like this work at the time. Ewing's response must have been something to effect of: "Oh boy, would I ever! If only I could get some money to do it." He had been trying to persuade oil companies for years to support such research but they showed no interest. They claimed the oceans would never be a productive source of oil, which goes to show that the marketplace doesn't always drive the best basic research.

The GSA gave Ewing a grant of a few thousand dollars and arranged for him to go aboard Woods Hole Oceanographic Institution's *Atlantis* for two weeks the following summer. Ewing, Vine and Webster set about devising an undersea seismic experiment—the likes of which had never been conceived, let alone attempted—and building the tools they would need to conduct it. They were able to obtain a seismic cross-section across the continental shelf of the Atlantic coast, which spurred them to devise ways to operate in deep water.

In Lehigh's machine shop, they hand-built geophones to receive vibrations from explosive shocks, an oscillograph and a camera to record them, tiny galvanometers to measure small electrical signals, amplifiers, special switches, timing devices and electrical motors, and a water- and pressure-tight container for the equipment.

No one had ever fired explosives in the ocean depths and Ewing had been advised that TNT would not explode when wet or at the seafloor, where the pressure approached 8,000 pounds per square inch (psi) and the temperature was close to zero degrees Celsius. They constructed cylinders out of foot-long artillery shell cases and poured in TNT that was melted on *Atlantis*' deck with steam piped from the engine room—in a device we called "Vine's Still." New detonators were devised for use in the sea. Electrical leads were insulated from seawater with hot tar. The tar was dispensed from an electric coffee pot parked on house shingles because the *Atlantis* crew didn't favor tar dropping on decks that they had scrubbed clean with holystones.

All the apparatus was strung on a cable: the main instrument chamber, four geophones spaced 100 feet apart and beyond the outermost geophone, three bombs at 1,000-foot intervals. A 1,000-pound lead weight was attached in front of the cable, which was attached to a wire rope that lowered the equipment to the seafloor three miles below. When Ewing and the crew believed that the farthest bomb was nearing bottom, the ship would get under way at slow speed to try to lay all the gear in a line on the bottom. The wire rope was paid out slowly to try to curtail any tugging on the equipment caused by the ship's drift and wave motion. All told, it took an hour to prepare the equipment and another three to four hours to lower it to the bottom.

Shortly after the equipment was in place on the seafloor, a preset timer switch started a motor that activated the amplifiers, lights and cameras inside the instrument chamber. Ten seconds later, the three bombs fired in ten-second intervals. The geophones received the sound waves reflected from subseafloor layers and sent an amplified signal to the galvanometers. That caused a light beam from a bulb to move back and forth along a slit opening in a special light-tight camera—thus recording the up and down motion of the ocean floor from seismic waves from the bombs. The wavy black lines were captured on a moving paper record operated by an electric motor in the camera.

To provide precise timing for the photo record, a tuning fork was equipped with a slit attached to each leg of the fork. A circuit caused the fork to vibrate in precise intervals; each time the fork vibrated the slits would line up with the camera slit and allow light to fall on it. By this means, seismic wave velocities could be estimated with an accuracy of about 0.001 seconds.

The experiment was completed in perhaps two minutes, then it took a good five hours to hoist and recover the apparatus. So an operation started at daylight wouldn't be completed until nearly dark. In the summer of 1937, Ewing conducted four experiments using this equipment. None was successful. Despite everyone's best efforts, the ship tugged too much on the line and distorted all the measurements.



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The GSA gave Ewing three more grants in 1938, '39 and '40, and in the summer of 1938 he was set to go aboard *Atlantis* to try again. With the GSA funds, he hired Vine and me at fifteen dollars a week during the summer and we worked eighteen hours a day, seven days a week. We found our wages hard to live on, so we bought a ten- by fourteen-foot tarpaulin and borrowed about six blankets from our lab supplies. We stowed these in the rumble seat of my Ford during the day and round about midnight, after work, we'd drive out into the countryside, find an empty field and make our bed, folding the tarpaulin in half and stuffing the blankets inside.

Normally we got up around six a.m. and were already working in the lab long before anyone was awake. But one night we worked later than usual and were so tired we laid our tarp-bed on the campus near the physics department. We overslept and woke up to find a campus cop, our friend, standing over us. The campus was alive with people moving about. Vine and I, in our underwear, struggled to get dressed in our cocoon, as the cop stood by watching us and laughing at our contortions.

Once again we had two weeks of ship time on *Atlantis* in 1938, essentially as guests on someone else's cruise. We were aboard on a not-to-interfere basis, which usually meant that we got no more than three or four days of operations at sea a year to conduct our experiments. We did everything we could to improve our instruments and methods during the year, then had one brief chance to see if they worked. Afterward we'd go back, analyze what might have gone wrong, rebuild things to try again during our two-week allotment the following year.

With ship time so precious, Ewing could not stomach wasting any of it, so while we waited our turn on the *Atlantis* in 1938, we put together an underwater camera, housed in a glass test tube about eight inches in diameter, a half-inch thick and four feet long. We affectionately called it the "Pyrex Penis." It had a watertight seal made out of a stretched inner tube. We made reflectors for the bulbs from coffee cans. It worked like a Pogo stick, hitting the

bottom to take a photo, bouncing up, hitting the bottom again to take a second photo and then releasing its ballast to float back up again.

Nobody thought it was worth the effort because they believed the seafloor was devoid of life. But even our primitive initial underwater photographs quickly dispelled that notion. The Pyrex Penis was a forerunner of numerous cameras we made over the years, with which we took thousands of pictures at all ocean depths up to 3,000 fathoms.

We built all kinds of things, innovating to make them more effective yet as cheaply as possible. To protect our underwater camera flashbulbs from shattering under pressure, we found that thick drinking glasses typically found in diners in those days made excellent transparent pressure vessels. We'd grind the lip flat so that it would seal with just a little grease and be held together by the pressure. We found that cans of fruit salad were just the right size to make covers for one of our instruments. So we all ate a lot of fruit salad and then we nickel-plated the cans so that they wouldn't rust in the ocean. Who ever heard of such a thing?

Because the bombs we used in 1937 were expensive and time-consuming to make, we conducted an experiment and successfully fired powdered TNT enclosed in an inexpensive heavy rubber weather balloon. The experts had been wrong: TNT *would* fire under temperature and pressure conditions on the ocean floor. (It also fired when wet, we learned quickly.) Our new cheaper bombs generally worked, though we did have many misfires. And once again we were plagued by the ship tugging on the instrument line. Sometimes the equipment was laid in a jumbled pile, probably because of unknown currents. Sometimes cables and instruments were damaged by being dragged on the bottom. On one occasion a bomb got caught in a loop of cable near the instrument case. Fortunately it had not fired, but we knew it that it was only a matter of time before a bomb would blow up an oscillograph. It became obvious that we had to find another way to do the experiment.

We returned from sea in September 1938, making an amazing twelve to fourteen knots under sail on *Atlantis*.

I was glad because I feared I would be late to register for classes in Lehigh the next morning. It turned out we were riding the forefront of the great hurricane of '38, the most devastating hurricane to hit New England this century. It took a full night and day to drive back to Bethlehem through a nightmare of howling winds and rains, washed-out roads and bridges, flying signs, downed trees and sparking power cables.

In the fall of 1938 Ewing received a Guggenheim Fellowship, which allowed him to take leave of his academic duties for a year to concentrate on his research. Ewing decided to design a new system so that the equipment didn't have to be laid along the bottom. In the new system, each geophone and its amplifying and recording equipment and each bomb and its firing mechanism would be built separately. Each of these was attached to a float and a weight and put into the ocean at appropriate distances along a line. The float was a large section of hose filled with gasoline. Beneath the instrument a bag filled with a block of compressed salt was attached to cast-iron weights. When the salt dissolved, the weights would be released and the floats and instruments would return to the surface. The size of the block of salt determined how long the instruments remained on bottom. We made extensive laboratory tests to find out how much salt to use.

The geophones were suspended from the oscillographs by a triangular frame of bamboo, with holes drilled into it so that the cavity would flood with seawater and not collapse under pressure. Each bomb also had a separate firing unit, each synchronized by suitably precise clocks. One of Ewing's former students worked at the Hamilton Watch Company and had invited us to compare its highly touted "Shortt Clock" with another highly accurate timepiece of the day, the Crystal Chronometer. Ewing had used the latter in making the first gravity measurements at sea as a guest aboard a Navy submarine.

The Shortt Clock was the standard by which all Hamilton watches were rated, and the signal for the Shortt Clock was piped throughout the watch-making factory. When we conducted our test, we found that the Shortt Clock was indeed accurate—except between four-thirty and

five-fifteen in the evening. When all the factory machinery started to shut down for the day, it suddenly altered the electronic load on the Shortt Clock, affecting its operation until about five-fifteen, when it got back in sync. Hamilton immediately took steps to correct a problem that would lead to false ratings on its manufactured goods. In appreciation, they gave us a dozen Hamilton pocket watches, the kind railroad conductors always used because they kept the best time of any small watch of that era. We quickly re-engineered them to serve as timers to synchronize our new array.

That fall we all got busy building individual oscillograph units and float systems. We also had to test the instrument cases to make sure they would not collapse at deep-sea pressures. Bethlehem Steel Co. gave us a surplus fourteen-inch naval artillery shell and we sank it sharp end down in a hole in the ground-floor room of our laboratory in the physics department. We mounted a one-ton hydraulic automobile jack on the back end of the shell, mating it with the hole in the shell's base, which had a very heavy plug to seal the explosive powder inside. Another former student of Ewing's, then working at Johns Manville, made us a cylindrical ring of rubbery asbestos, which served as a gasket between the plug and the shell. This was probably the first O-ring seal. By pumping in oil with the jack, we could raise the pressure in the shell up to 10,000 psi and pressure-test our equipment.

We also worked on the bomb misfiring problem. We believed that pressure was collapsing the detonators and causing the misfires. We decided to try putting the detonator inside a brass tube, figuring that under pressure, powdered TNT would pack around the detonator and form a seal that would prevent it from collapsing. We constructed such a detonator and put it in our pressure shell to test it late one night. When the pressure reached 4,000 psi, there was a loud thump and the pump stopped working. Vine and I were examining it to see what was the matter when Ewing, who had been working in his office upstairs, came running in. Noting that the pressure was rising, he told me to open the pressure release valve, and a fountain of burnt oil and cinders spurted out. We all left the lab posthaste.



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When we got to the far end of the hallway, we realized that the campus policeman was due by soon. He often looked in our lab window and paused to chat awhile with us. I ran back into the lab, pulled down the window shades and jumped out the window rather than pass close to the pressure vessel again. A half-hour later, we returned to the lab. It was a mess. Burnt oil and cinders coated the ceiling, walls and furniture. Vine and I spent all night cleaning the lab so that it didn't look too much worse than usual by the time the department staff arrived the next morning. We finished cleaning it over the next two days. We found oil and cinders inside small cardboard boxes, which were inside cigar boxes that were stored inside cubby holes. How the mess could have so thoroughly distributed itself is still a mystery to me.

When we looked inside the pressure vessel, we discovered that the detonator tube had collapsed, causing the TNT to burn—but not explode. We filed this away and this failed experiment turned out to be a great boon a few years later when we had to devise detonators for our research during World War II. But at that moment, the near-mishap intimidated our detonator efforts and instead Ewing turned to friends at the DuPont company to build us detonators we could use.

All of our instruments had to be completed and tested by Thanksgiving weekend, when Ewing was scheduled to sail to Bermuda to meet Woollard and test the new equipment. To make that deadline, we usually worked every day until midnight in our lab. We couldn't afford to wait in line for machinists in the better-equipped physics department's machine shop to build things for us, so some nights we picked the lock, sneaking in to use the machines ourselves. Suspecting us, the head of the shop set traps to catch us, purposely leaving a few stray chips around the machines. We figured that out and brushed the chips into a dustpan and put them aside until we finished our work. Then we'd clean up the machine and put the chips back right where he had left them. He never got wise.

Vine, Webster and I worked every minute we weren't in classes to get the equipment in shape, but as Thanksgiving

approached, it wasn't ready. We worked continuously from Wednesday evening until Saturday morning. We loaded all the equipment in Floozey Belle, my Ford and Webster's Packard and started for New York at eleven a.m. Ewing's ship, *The Monarch of Bermuda*, was scheduled to leave two hours later.

The trip would have taken two hours on clear roads, but a deep snow had fallen on Thanksgiving. On icy roads we drove seventy miles per hour most of the way. At some traffic lights, Ewing would try to stop. Instead of stopping, however, he would go into a skid, turn around and often pass stopped cars, going backwards. I would pass them on one side in a full skid and Webster would follow, skidding past on the other side. Without stopping we'd regain control and keep going. Fortunately, we never hit anyone or each other.

But despite our efforts, when we arrived, *The Monarch of Bermuda*—with Ewing's wife and son on board and Ewing holding their tickets—was slipping her lines, tooting her horn and moving away from the dock. Doc tried, unsuccessfully, to get the authorities to hold the ship. Later, he told us that, in desperation, he had contemplated shedding his coat and trying to haul himself on board hand over hand on the last remaining line connected to the dock. But at that moment, the line was thrown overboard.

Ewing arranged to leave a week later and tested the new free-floating instruments off Bermuda. They worked, but Ewing had difficulties finding the equipment after it surfaced, so we made adjustments, adding two more buoys to make the line longer and easier to find.

We tested the adjusted system in the summer of 1939 aboard *Atlantis*. After we heard the bombs fire on the seafloor, we would begin to look for buoys returning to the surface from the undersea units. Vine and I would climb to the top of *Atlantis*' mast 100 feet above deck, using binoculars to search for the buoys in the open sea. We would stand on the spreaders—beams of wood perpendicular to the mast that were six inches in diameter and six feet long. From the masthead, a wire rope less than an inch in diameter passed the end of the spreader and fell to the deck, where it was attached to a rail. At first we must have bent the wire rope,

we held on so tightly. But after the first two hours, we got quite accustomed to it and became quite casual with our grip. Most times we would find the line of buoys while there was ample daylight, but sometimes it would take the rest of the day and we'd recover them well into dusk.

We proved the technique for placing gear at the bottom and recovering it, and our bombs fired properly. But we got no data. Our oscillographs failed to operate, even though they would operate perfectly in the laboratory on *Atlantis* just before a lowering and just afterward. We used little motors from electric trains to pull the paper through the oscillographs and we speculated that the motors just didn't work at seafloor temperatures approaching the freezing point. We were frustrated, but we were working on a tough problem and we didn't expect miracles. We didn't expect everything we tried would work right the first time.

The following July, in 1940, we tried again on our two-week *Atlantis* allotment. Once again we placed our equipment on the seafloor several times and once again the oscillographs failed. We were discouraged, but determined to persevere until we succeeded. Columbus Iselin, the new director of Woods Hole Oceanographic Institution, took pity on us and gave us an additional two weeks on *Atlantis* at the end of the summer.

On that cruise, our failures continued until the last lowering, when one oscillograph pulled the photo paper through and we got one record of four shots. This happened because of a mistake I had made. Before we sent gear to the deep, I would test it to be sure it was working. Then I would replace the test batteries with fresh ones. In one case, I simply forgot to switch batteries, and that's the one that worked. I had used Eveready batteries for the test and Burgess batteries for the experiment. It had never occurred to any of us that battery brands made any difference, but though both worked at surface temperatures, only the Eveready battery could provide ample current at low temperatures. My error had serendipitously solved a major problem! Unfortunately, our available time at sea was gone for that year, but we now knew how to proceed in the future.

We got ready to return to Lehigh, but we had one piece of unfinished business. Next door to WHOI was a service station that a benefactor had set up in his will to provide for a faithful longtime chauffeur. The garage was equipped with a large lathe, a drill press and a miller—all hardly used. The garage was flooded during the '38 hurricane and these machines were essentially abandoned when residual salt severely rusted their mechanisms. For years, we had craved our own machinery so that we could work as long and as late as we wanted to, without having to wait in line or sneak into the Lehigh machine shop after hours. Vine offered the garage owner a few hundred dollars for the rusted machinery and persuaded Ewing that the expenditure would allow us to get our work done more quickly.

We hired a dump truck to transport the major parts of the machine tools to Lehigh. Vine and I removed all of the machines' more delicate parts and packed them into Floozey Belle. Fully loaded, her springs were so compressed that her rubber bumpers rested on the axle housings. The last item to go in, just leaving us a minimum of sitting room up front, was the lead screw for the lathe. Laid on top, it extended from the front seat to the back. Vine said, "Well, we got it all in," and slammed the back door. There was a shattering noise and the lead screw stuck out about an inch through a small round hole in Floozey Belle's "shatterproof" door glass.

We drove carefully, coming almost to a complete stop to negotiate any bumps on roads that had suffered through the Depression. Vine and I spent the week before classes cleaning all the rust off the machines and oiling, greasing and reassembling them. We had just finished when Ewing called us from Woods Hole. He invited us to join him in an imminent program of wartime research at Woods Hole initiated by the newly formed National Defense Research Committee (NRDC). We hired another dump truck, loaded up the machines and set them up in a basement at WHOI, thus establishing that institution's first machine shop. Those three heavy machines had a ride of more than 800 miles to get across the fence at



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Woods Hole. They were crucial tools for our work during World War II.

On the brink of war, the Navy was quite interested in the transmission of sound through the ocean—an essential component for submarine and mining operations. The military had conducted several experiments and collected a lot of data but could not interpret them. We had learned a great deal about the physics of sound transmission through water from our seismic work, which had involved timing sound waves from their explosive sources to our receivers.

Ewing and I set about interpreting the Navy's data. All told, ten thousand calculations had to be done to figure out and plot sound velocities versus distances, depths, pressures, salinity and temperatures. I started on the work one evening after dinner. Ewing came in, saw how tedious the work was, and sat down to help. When we finally finished at six o'clock the next morning, Ewing looked at me and said, "You know, Worzel, if you hadn't finished this by yourself by midnight, I would have given you hell." He probably would have.

We revealed some of the fundamental physical properties of how sound operated in the oceans. That's where Iselin came in. As an oceanographer, he had a handle on how the ocean operated. So we combined our expertise and by December we had written up an operational manual on sound transmission in the ocean, which the Navy still used well into the 1960s.

For example, we found that sound waves from a surface source (like a ship sonar) would essentially split in surface waters, with some bending horizontally toward the surface and others bending vertically toward the deep. In between was what we called a "shadow zone," where sound waves did not travel and where a submarine could remain undetected by sonar.

Deeper down, we found that sound appeared to bounce off a layer in the ocean called the thermocline, where temperatures dropped sharply—almost as if the sound waves were striking something solid. We instantly realized the practical value of this discovery: Submarines

could more easily escape detection from surface ship sonars by diving beneath the relatively sound-proof ocean layer. So during this same period, Ewing and Vine redesigned the bathythermograph, or BT, which measured water temperatures versus depths. These measurements could determine the thermal layering in the water column. Unlike the original BT designed by WHOI's Athelstan Spilhaus, Ewing and Vine's BT sank rapidly and responded within a second rather than five minutes, making it an altogether much more efficient and usable device. Vine and I made the first ten of these new BTs in WHOI's machine shop by early January of 1941, and at age twenty-one, I was sent out as chief scientist on *Atlantis* to test them. BT measurements were used by ship commanders to space escorts around convoys most efficiently. We set up training programs for Navy BT operators and interpreters.

We did all this work without getting paid because no government funds had yet been appropriated. A WHOI employee named Al Woodcock, a bachelor, let us live in his house, though we were probably a great nuisance to him. We pooled any money we had for food. Then Iselin managed to scrounge a \$1,000 grant for the three of us to live on until an NDRC contract finally came in at the end of January. Then we were able to pay our "back rent."

About a year later, Ewing and Vine designed an instrument for submarines that would give them the same information that the BT gave surface ships. Submariners used this instrument to avoid detection. There are numerous accounts of how these submarine BT's saved subs and lives. Later still, Vine added lines on the BT chart that guided diving officers on the amount of water to flood or pump to maintain their sub's balance. This changed diving from an art to a science. (After the war, Vine's interest in submarines continued and he had a long, illustrious career at WHOI. The institution's famed research submersible *Alvin* is named after him.)

Our initial, highly useful discoveries prompted the Navy to continue our broad mission throughout the war to learn anything and everything we could about sound

transmission in the ocean. That, of course, meant using explosives to create the sound, and safety was a pre-eminent concern. We proceeded to explore sound transmission below the thermocline, and we needed to fashion bombs that would explode at 800 fathoms.

Our first bombs employed standard electrical detonators with long external circuits to ensure that the circuit was not completed before the bomb had reached a safe depth. Still, a short circuit or a sudden jolt conceivably could set them off. We turned the problem over to the Navy's Bureau of Ordnance to design safer bombs. Time passed and we were ready to start our research. We asked the bureau for our bombs and learned that it hadn't even started working on them.

I was incensed. I remember saying to Ordnance officials, "What's so damn difficult? You could design a safe bomb in a few days and test it in a week!" They replied, "Well if you can do it so easily, go right ahead."

It was then that we remembered our little mishap in the pressure vessel in the Lehigh lab. We recalled how the TNT burned but did not explode and we set about designing a non-electrical detonator that could be activated with fire. That was relatively easy; the harder part was finding something to start the fire.

The first thing we thought of was match heads, from the kind of match that you could strike off your fingernails or shoe. I cut the heads off and attached them to a piece of cellophane tape. It worked. But cutting and pasting individual detonators with matches and tape seemed inefficient. We asked match manufacturers if we could buy some of their raw slurry, but they didn't want to get into the side business of selling dangerous chemicals. Doc asked his friends at DuPont to help and he arranged for me to go down there to discuss the problem. On the way to the train station in Woods Hole, a little kid put his toy cap pistol in my belly and said, "Bang, bang, you're dead, mister." On the contrary, I came alive. I grabbed the kid by his shoulders and said, "Where did you get those caps?" We found the store and bought some caps. They worked perfectly to fire

our detonators and they were very safe. I put these new cap detonators through torture tests, hitting them with hammers, dropping them off a three-story building, but they wouldn't fire prematurely.

I had to write a memo about the detonators to the Bureau of Ordnance and noted that they cost less than a dollar to make, compared to the electrical detonators, which cost about \$150. I hadn't meant to rub their faces in it, but I guess we had showed up the Bureau and they were irritated. The Navy insisted that our detonators had to fulfill its safety requirements and demanded to know the contents of the caps. We called the cap manufacturer and informed our Navy liaison officer. Not having too much experience with cap ammunition, the Navy called the Bureau of Mines. Our liaison officer overheard the conversation and told us later what the Bureau of Mines official told the Navy: "That stuff's so safe, it's used to make caps for toy pistols!"

We went on to make thousands of "Woods Hole detonators," as we called them, for our work during and after the war. One of the more important things we learned was that low-frequency sound traveled much greater distances in the ocean than high-frequency sound. We worked with Chaim Pekeris, a brilliant theoretician at Columbia, giving him field data to prove his otherwise untested theories on sound transmission through shallow water. Together, we solved a vexing problem for the Navy, showing how the Germans were using fast-moving, low-frequency sound traveling in bottom layers to outsmart acoustic mines and blow up mine fields that took the Navy days to lay. The Navy made the necessary adjustments.

We also explored the SOFAR (sound fixing and ranging) channel, though it didn't have a name when Ewing predicted its existence in 1943. The narrow channel was bounded, top and bottom, by layers where water velocities increased—creating sort of a floor and ceiling off of which sound waves bounced back and forth and onward through the channel. Instead of dispersing throughout the vast ocean, the sound waves were essentially caught in this



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channel and could only travel horizontally along it—for long distances before their energy was absorbed.

Ewing proposed that that SOFAR channel could be used for rescues of downed aircraft or submarines in distress. The submarine could release a bomb that would explode in the channel and aircraft could be equipped with similar bombs. By triangulating the sound signal received at three faraway listening stations, officials could pinpoint the sub's or plane's position and send swift rescue in the open seas.

However, authorities fretted about placing an explosive of a few pounds on subs and planes (though, ironically, carrying thousands of pounds of highly combustible fuels did not seem to worry them). Nevertheless, Roger Revelle at the Bureau of Ships (and later director of Scripps Institution of Oceanography) saw merit in the proposal and authorized an experiment in 1945.

We lowered a hydrophone to 800 fathoms, at the axis of the SOFAR channel, and sent a ship away firing four-pound charges at fifty-mile intervals. When the firing ship reached a distance of 1,000 miles, the signals were still coming in strong. We asked them to continue, but their orders would not permit this. Apparently the Navy had judged that we would have long lost the ability to hear the shots at such a far distance. In a later experiment we heard sound signals 2,300 miles away, even though the sound had to cross the Mid-Atlantic Ridge, which partially blocked the sound channel. The SOFAR charges came in loud and clear on small loudspeakers in the scientific quarters near the aft end of the ship. They were so loud that the cook could hear them clearly in the galley at the other end of a 120-foot-long ship.

Later, we deployed three hydrophones in the SOFAR channel: one on the seafloor near the island of Eleuthera in the Bahamas, one from *Atlantis* anchored in the Sargasso Sea 1,200 miles southwest of Bermuda, and one from a ship called *Saluda* at a fixed position 100 miles southwest of New York. A third ship was sent across the Atlantic firing bombs every six hours that would go off near the surface and in the channel. Analyzing the sound waves received at

the three hydrophones, we fixed the locations of the firing ship more accurately than officers aboard the ship could pinpoint their own location with celestial navigation!

Toward the end of the war, Ewing was offered a professorship at Columbia and I went with him to enroll in graduate school. After all, I only had a B.S. degree at the time. Ewing had his first crop of graduate students—me, Frank Press, William Donn, Rene Brilliant, Gordon Hamilton, Nelson Steenland and Ivan Tolstoi. We occupied a few hastily refurbished rooms in Schermerhorn Hall on the Morningside campus, outfitted with government surplus desks.

In one room was a trapdoor that led to a small room that had been hollowed out of Manhattan Schist. There we planned to make earthquake seismic observations. Two rooms were set aside for a machine shop, because we had become so accustomed to building our own instruments. We talked Woods Hole out of one of our old naval shell pressure-testing devices that we had used during the war.

Ewing hired Angelo Ludas, who had worked at Columbia in the Manhattan Project during the war, to be our shop foreman (and total work force). Ludas equipped our shop, making good use of government surplus lists. He fit our style seamlessly and quickly became an essential part of our team. He had that "can-do" spirit and probably was the most naturally mechanically knowledgeable person I've ever met in my life. We worked together for the next twenty-five years, and there was no end to the things we built—from electrical switches, clamps, gussets and gaskets that improved the operation of our equipment to prototype instruments that had never been made before. He was one of a kind, so much a part of the fabric of Lamont that the observatory would not have been the same place without him.

By the summer of 1948, we were already getting too big for our quarters and requested more space, to no avail. We even went so far as to design a new building for ourselves between Schermerhorn and Columbia's powerhouse. About then, Ewing had received an offer to establish a geophysics research group at the Massachusetts Institute of Technology and to bring all his students there. MIT offered us a former

estate near New Bedford to house our operations. Ewing took Press, Hamilton, Steenland and me to tour the grounds and discuss the issue.

Upon our return, Ewing talked with Dwight Eisenhower, who was then Columbia's president, and Paul Kerr, chairman of the Geology Department. They countered MIT's proposal by offering us an estate across the Hudson River in Palisades, New York, which was about to be donated to Columbia by Florence Lamont, the widow the financier Thomas Lamont. Columbia would accept the gift if it did not become a financial burden to the university, and Kerr promised to raise \$200,000 to get us going. He and Eisenhower persuaded mining companies to provide the funding. Without Kerr's effort, Lamont never would have gotten off the ground.

The same group that went to MIT also toured the Lamont estate. We debated the pros and cons and Ewing put it to a vote. Unanimously, we agreed to stay at Columbia. In late December of 1948 Columbia received the deed for the property. Not long afterward, Press and I were setting up a seismometer on the floor of the estate's empty swimming pool. Press was thrilled that we would not have to contend with the vibrations from subways and trucks on Broadway. We were all thrilled by all the new space. The estate had 125 acres—actually more like 135 acres, but we had to give up ten acres that lay across the New Jersey state line. Robert Moses, New York's infamous road, bridge and parks builder, wanted the ten acres for his new Palisades Interstate Park system. Columbia wanted to close West 116th across its campus from Broadway to Amsterdam Avenue. They struck a deal.

In 1949, Hamilton and I chose a location for a field station in Bermuda to continue our research on the SOFAR channel. It was equipped with a geophone planted 800 fathoms down on the seafloor and capable of detecting very low-frequency sound. The station came in very handy when I was invited to a meeting at the U.S. Naval Submarine Base in Groton, Connecticut.

The Navy was interested in finding ways to locate submarines in deep water over greater distances. At the

time, sonar techniques hadn't advanced much since World War II and subs couldn't be located farther than two miles away. I had only just completed my Ph.D., so I waited until the end when I was asked my opinion. I told the group that during the war, listening ships reported hearing low-frequency sounds of convoys 100 miles away. These reports were dismissed as improbable, but Ewing and I believed them because our work had demonstrated that low-frequency sound could be heard at great distances in the ocean.

We proposed a test at the Bermuda Station. Over a week, two submarines alternately sailed in the area and we monitored the sounds they made. With our prior experience and research, we were able to track the subs to a range of eighty to 100 miles. It was a breakthrough, increasing detection ranges by a factor of forty to fifty. Soon after the Navy founded Hudson Laboratories at Columbia to pursue low-frequency sound research. Developments followed quickly and the Navy set up its Sound Surveillance System (SOSUS) to listen to all sorts of ship traffic around the world. The Bermuda experiment also pinpointed a particular sound frequency that allowed us to follow the subs over great distances. The Navy soon tracked that noise to the firing rate of pistons in the sub's diesel engines. A whole engineering science emerged, and continues today, to subdue submarine noises.

Over the years, aside from its continuing basic research on the SOFAR channel, the Bermuda Station also helped the Navy in other important ways. When the submarine *Scorpion* mysteriously sank, the station helped pinpoint where it went down so the Navy could find out why. The station also provided a means to locate very precisely where test missiles (equipped with sound sources that exploded in the SOFAR channel) landed, which helped the Navy assess the accuracy of its missiles. After the student disturbances at Columbia in the late 1960s, the university stopped doing classified military research. Several of us created a private non-profit entity, Palisades Geophysical Institute, and continued to use the Bermuda Station for important research for the Navy.



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Our next important step was to acquire our own ship, so that we could control our own research destiny and no longer be dependent on other people's ships, as we had been for fifteen years. We accomplished that in 1953 by buying the *Vema*. As you will learn in a subsequent chapter, that took a lot of determination and little bit of hustling, but we were quite used to that.

So it went. Others in this volume will pick up the story from here. We simply continued doing what we had been doing—pursuing any and all interesting scientific lines of inquiry that we encountered; designing and building any instruments that we needed and constantly improving or fixing them; working long hours; collecting data religiously and relentlessly; seizing any opportunity to advance our work; wasting little time and money; never giving up or in; solving any problems that arose. The pattern had been established and so had my role. For a decade, it had been my job to find ways to get things done, no matter what the situation. For the next three decades, working with Ewing at Lamont and later at the University of Texas, Galveston, I used my skills and imagination to solve problems—whether those problems were electrical, mechanical, scientific, logistical or administrative.

I remember one time I was testing a new stable platform to make better gravity measurements from surface ships. About half-way across the Atlantic, one of the gravimeter's transformers burned out so I could no longer make measurements. I cabled for someone to send a replacement transformer to meet the ship in Naples, but sitting idle in the meantime just wasn't our style. I took apart the transformer and unwound all the wires in its four windings, counting the number of turns it took. Aboard that cruise were many representatives and engineers from big engineering firms that were seeking to sell gear to the Navy. They all shook their heads at me, saying, "You don't really think you can rewind that transformer, do you?" I said, "The people who made the first transformers wound them by hand, so why can't I?" I found some new wire in the ship's shop, rewound the transformer, and got it operating again.

All those big-shot engineers were shocked. But all my experience, all my years at sea, had taught me that you can do all sorts of things that you would have thought impossible. If you don't have the proper equipment and there's no hope of getting it for a month, but you *have* to have something, you find a way. I used to call a hand file a "portable miller," because you can accomplish pretty much anything with a file that you can with a big machine shop miller. It just takes persistence and hours of sweat and toil to do it.

I recall one time a professor at Cambridge said to Ewing and me: "Well, of course, you people in the States are getting remarkable results because of all the money you have to work with. We don't get much money over here so we can't do these things." Doc looked at him and said, "Have you ever built any of your own galvanometers from scratch? Have you built any geophones? We did."

And that's pretty much how the Lamont-Doherty Earth Observatory was built. We started with next to nothing and created a preeminent research institution that forever changed our understanding of our planet.



*"I had a blank canvas to fill with extraordinary possibilities, a fascinating jigsaw puzzle to piece together. . ."*





# CONNECT THE DOTS

## *Mapping the Seafloor and Discovering the Mid-Ocean Ridge*

by Marie Tharp

Not too many people can say this about their lives: The whole world was spread out before me (or at least, the seventy percent of it covered by oceans). I had a blank canvas to fill with extraordinary possibilities, a fascinating jigsaw puzzle to piece together: mapping the world's vast hidden seafloor. It was a once-in-a-lifetime—a once-in-the-history-of-the-world—opportunity for anyone, but especially for a woman in the 1940s. The nature of the times, the state of the science, and events large and small, logical and illogical, combined to make it all happen.

Right up until World War II, all that water—a few miles deep and hundreds of thousands of miles across—proved an ample barrier, preventing humans from getting any picture of what lay at the bottom. On any map of the world, three-quarters of the Earth was a uniform, featureless blue border for the continents. Scientists thought the ocean floor was almost as featureless—a flat, unchanging plain, a dumping ground slowly filled by sediments eroding from land.

Early depth measurements, collected using ropes and lead weights such as cannonballs, suggested that the ocean floor was slightly more complex, however. With 200 soundings obtained in this way, the Navy's Matthew Fontaine Maury marked a plateau in the middle of the North Atlantic on his 1854 map. In the 1870s, spot soundings taken during the legendary HMS *Challenger* expeditions hinted at a broad rise in the central Atlantic, and temperature measurements by the *Challenger's* expedition leader, Charles Wyville Thomson, indicated that

there was a barrier between the east and west basins of the Atlantic.

In early 1947, Doc Ewing undertook a Sigma Xi lecture tour with the official purpose of finding bright students to work in oceanography. Actually, he was scouting for a group of technicians from wealthy families to whom he could offer adventure instead of pay. After Ewing's talk, Bruce Heezen, who was then a junior at the University of Iowa, introduced himself to Doc, who said, "Young man, would you like to go on an expedition to the Mid-Atlantic Ridge? There are some mountains there, and we don't know which way they run."

The following summer, Bruce went to the Woods Hole Oceanographic Institution to join Doc on an expedition on *Atlantis*, using a continuous echo sounder to take profiles of the seafloor in the North Atlantic. But Bruce didn't get to go with him. Instead he got his own ship, *Balanus*, serving as chief scientist, even though he was not yet a senior in college. He got some great on-the-job training, went back to Iowa in the fall to finish his degree and then joined Doc at Columbia.

My course to Lamont was a little more indirect. My father, William Edgar Tharp, was a soil surveyor for the U.S. Department of Agriculture, Bureau of Chemistry and Soils. Papa was a field man and his assignments were to make a soil map of a county, produce a written report describing the soil types and recommended uses and to collect soil samples for analysis in the chemistry division. These were



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printed by the government and distributed to farmers, insurance companies and the university extension divisions. We were constantly on the move, with Papa working in the Southern states during the winter and the Northern states in the summer. By the time I finished high school I had attended nearly two dozen schools and I had seen a lot of different landscapes. I guess I had map-making in my blood, though I hadn't planned to follow in my father's footsteps.

Throughout our travels, Papa always told me, "When you find your life's work, make sure it is something you can do, and most important, something you like to do." In college at Ohio University, I changed my major every semester. I was looking for something I was good at, something I could get paid for, and something I really liked, but there weren't many opportunities for women then, except as a teacher, secretary or nurse. I couldn't type and couldn't stand the sight of blood, so I decided to try teaching and began taking education courses, which convinced me that I wouldn't like teaching all that much. I graduated with majors in English and music and four minors.

I never would have gotten the chance to study geology if it hadn't been for Pearl Harbor. Girls were needed to fill the jobs left open because the guys were off fighting. A year after the war started, the geology department at the University of Michigan opened its doors to women. In 1943 about ten of us girls responded to one of their fliers, which promised a job in the petroleum industry if we got a degree in geology. It seemed like something I could do. I earned a master's degree and got a job with Stanolind Oil and Gas Co. in Tulsa, Oklahoma. Some of the girls I went to school with went into micropaleontological work and spent their time looking through microscopes. That seemed tedious, so I went to the University of Tulsa and got a degree in math. Still searching for something more challenging, I went to New York in 1948.

I looked for work at the American Museum of Natural History, but I decided I didn't want to work there after a paleontologist told me how it took two years to separate a fossil from the surrounding matrix. I couldn't

imagine devoting so much time to something like that, so I tried Columbia to see if I could get a more interesting research job.

Just because I had a math degree, they sent me down to see Doc Ewing, but he was at sea. I went home and waited three weeks for him to come back. When he heard about my background, he was surprised and didn't know quite what to do with me. Finally he blurted out, "Can you draft?" I had had a part-time drafting job at Michigan, so he hired me.

About two weeks later, Bruce arrived at Columbia. At first I worked for anyone who needed me. But after a few years Bruce kept me so busy that I ended up working exclusively for him, drafting and plotting ocean floor profiles.

During World War II, Ewing and Joe Worzel, working at Woods Hole, had developed the continuous echo sounder for the Navy. With this new instrument, depth measurements could be made nonstop and round-the-clock. A sound signal, usually an electronic ping, would be sent out at a regular interval, and a microphone inside the hull of the ship would pick up the echo. As a ping was sent out, a stylus would be set in motion downward across a continuously spooled strip of four-inch-wide paper. When the echo returned, the stylus would mark the recording paper by burning it with an electric spark. The result was an uninterrupted profile of seafloor depths along the ship's course. Relatively uninterrupted, that is: The echo sounder depended on the ship's electric power, which went off whenever someone opened the ship's refrigerator. When that happened, no echo returned and the sounder recorded depths as bottomless as the crew's appetite.

With technological advances and Ewing's drive and direction, tens of thousands of depth measurements in the North Atlantic had been obtained from 1946 to 1952 on cruises of *Atlantis*. We also had some data from *USN Stewart*, which in 1921 was the first Navy ship to make a continuous track across the Atlantic. We had interminable rows of sounding numbers that I was supposed to turn into highly detailed and complete seafloor profiles.

Bruce and Ivan Tolstoy at Lamont had devised sheets on Mercator projection to plot surrounding data at a scale of 1:1,000,000, creating the standardized system that is still used today by the Navy and Lamont. Plotting on these sheets, Hester Haring and I went to work in 1952 at drafting tables in a lab on the second floor of Lamont Hall, near Bruce's office with its coveted private study (a former Lamont bathroom). First, Hester would plot the depths from the sounding data. Then we plotted profiles with significant selected depths along the ship's course. The profiles had to be drawn in a consistent manner. Any mistakes and someone like Bruce or I would scrawl a message like, 'Plotted Backwards!' on the profile and have it redrawn. Bruce and I would then compare the depths on the profiles with the original soundings.

Eventually, after the plotting, drawing, checking, correcting, redrawing and rechecking were done, I had a hodgepodge of disjointed and disconnected profiles of sections of the North Atlantic floor. Plotted on a map, the ship's tracks looked like a spider's web, with the rays radiating out from Bermuda, where most of the research vessels took on supplies and water. Sometimes, the tracks zigzagged, as the ships fled from the paths of storms.

After another six weeks to arrange and piece together the profiles in proper order from west to east, I completed six more-or-less parallel, trans-oceanic profiles of the North Atlantic. I noticed immediately the general similarity in the shape of the ridge in each profile. But when I compared the profiles, I was struck by the fact that the only consistent match-up was a V-shaped indentation in the center of the profiles. The individual mountains didn't match up, but the cleft did, especially in the three northernmost profiles. I thought it might be a rift valley that cut into the ridge at its crest and continued all along its axis.

When I showed what I found to Bruce, he groaned and said, "It cannot be. It looks too much like continental drift." At the time, believing in the theory of continental drift was almost a form of scientific heresy. Almost everyone in the United States thought continental drift was

impossible. Bruce initially dismissed my interpretation of the profiles as "girl talk."

But I thought the rift valley was real and kept looking for it in all the data I could get. If there were such a thing as continental drift, it seemed logical that something like a mid-ocean rift valley might be involved. The valley would form where new material came up from deep inside the Earth, splitting the mid-ocean ridge in two and pushing the sides apart.

Soon afterward, almost on impulse, we decided to make a physiographic diagram of the ocean floor in the style of A.K. Lobeck, professor of geomorphology at Columbia in the 1920s. Unlike flat contour maps, physiographic maps show the terrain as it would look from a low-flying plane. By 1952, Bruce had been on enough cruises to know most of the features of the Western Atlantic. So, after about an hour of doodling, he produced our first diagram. He was somewhat unhappy with his effort and asked me to do it over. But both of us were pleased with the technique. It allowed us to capture the seafloor's many textured variations, contrasting the smoothness of the abyssal plains, for example, with the ruggedness of the mountains along the ridges. But we also had an ulterior motive: Detailed contour maps of the ocean floor were classified by the U.S. Navy, so the physiographic diagrams gave us a way to publish our data. In retrospect, our choice of map style turned out to be significant because it allowed a much wider audience to visualize the seafloor.

I started using the physiographic technique to make a more detailed map of the North Atlantic. Our goal was to present it as it actually existed and as it could be seen if all the water were drained away. But, of course, there would never be enough ship tracks to do this. In the face of a minimum amount of data and the immensity of the world ocean, Bruce took a logical and multidisciplinary approach. We used data from wherever we could get it, from different disciplines and different sources, but took great care to ensure that these data from various sources were all plotted on the same scale. We used hypotheses of ocean floor structure to fill in areas



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where we had meager data. Our final guideline was that the sketching began from the shoreline seaward and from the mid-ocean ridge landward—that is, from the areas that we were most familiar with to those that we weren't.

More and better data accumulated. By 1952 Lamont had acquired the *Vema* and had installed on it the precision depth recorder (PDR), invented by Bernard Luskin at Columbia in a hole in the floor of Schermerhorn Hall. The PDR provided much more accuracy than earlier echo sounders, allowing us to differentiate between smoother and rougher-textured areas and to pick up more subtle seafloor features, such as seamountlets, scarps and sediment drifts. By *Vema*'s twentieth cruise, the precise sounding data were combined with highly accurate ship tracking, thanks to Joe Worzel, who installed a satellite navigation system on *Vema*, the first ever on an academic research vessel.

34 | Every other day, the captain of the *Vema* would read off soundings from the PDR records as the first mate plotted them along the ship's navigation track. Bruce had always insisted that soundings be read at every peak and valley and at every significant change of slope, rather than at equal time intervals of, say, fifteen minutes. The latter would have been easier to do, but it would have tended to miss small seamounts, scarps or canyons. When each chief scientist completed his cruise and was replaced by a new one, he debarked with a roll of sounding data.

Hester Haring, with her meticulous handwriting, using a crow quill pen and India ink on blue linen, maintained the *Vema* sounding records on standard 11:1,000,000 sheets for many years. These sheets became the bible to which we compared all other institutions' ship data. *Vema* data were classified as 9 on a 1-9 scale. Less precise data received lower grades, which were labeled with large red numbers on sheets that began bulging in our ever-accreting files. When laying several sheets from different places on a light table, we used these numbers to evaluate soundings quickly and to use them wisely.

While this work was going on, Bruce got involved in another project that provided another crucial source of data.

He and Doc had proved the existence of turbidity currents—slurries of sediment and water that behave as discrete streams within the ocean. They documented that a 1929 earthquake off the Grand Banks had precipitated turbidity currents of such high speeds that they snapped trans-Atlantic cables. Bell Laboratories was interested in laying new cables and asked Bruce to help determine the best locations for them. Bruce hired Howard Foster, a deaf graduate of the Boston School of Fine Arts, to plot the location of recorded earthquake epicenters in the oceans. In this pre-computer era, Howard had to plot tens of thousands of earthquakes by hand. While I was at my map table, plotting the position of the Mid-Atlantic Ridge and the alleged valley, Howard sat at an adjoining table making the map of oceanic earthquake locations. Both maps were created on the same scale, as Bruce insisted.

The earthquake epicenters weren't as precisely located as our sounding data. Their positions could sometimes only be located anywhere within an abominably wide range of several hundred miles. But when Bruce accounted for this, he noticed that a nearly continuous line of earthquake epicenters ran down the center of the Mid-Atlantic Ridge. Of course, Beno Gutenberg and Charles Richter earlier had noticed that a belt of shallow earthquakes followed the ridge, but Bruce saw that the earthquakes fell within the rift valley. Because all our data were on maps of the same scale, we could superimpose the maps on a light table, and when we did, the earthquake epicenters lined up within the valley. By then, I was certain that the rift valley existed. Bruce had remained skeptical. It was not until the middle of 1953, about eight months after I had worked up the first six profiles, that he accepted the idea.

Recognizing the validity of the correlation between earthquakes and the rift valley, we plotted the position of the valley by using earthquake epicenters for locations where there were no soundings. The extension of the valley into the narrow Gulf of Aden and landward into the Rift Valley of East Africa convinced Bruce in mid-1953 that the Mid-Atlantic Ridge was part of a gigantic 40,000-mile-long

mid-oceanic ridge system that extended throughout all the world's oceans. In fact, the mid-ocean rift valley takes its name from the terrestrial rift valleys of East Africa. We made profiles of some of the valleys in East Africa and noted the topographical similarities between the valleys in the ocean and on land. Bruce also noticed that the shallow earthquakes associated with the East African Rift fell within the valley walls. He began to endorse the existence of a continuous central valley within the mid-oceanic ridge.

Doc began to get interested at this point. He'd heard of this "gully," as we called it, and he would pop into our lab from time to time and ask, "How's the gully coming?"

Meanwhile, I had extended the Mid-Atlantic Ridge and rift valley into the South Atlantic, using data from another legendary oceanographic expedition, the thirty trans-South Atlantic cruises of Germany's *Meteor* in 1925-27. Sounding data from those cruises would have confirmed right then that the Mid-Atlantic rise extended into the South Atlantic and that it was not broad and gentle, as Maury and Thomson had thought, but narrow and extremely rugged. But the discovery had remained hidden in the unanalyzed data as scientists at the time focused on physical oceanographic measurements of currents and seawater properties, rather than on the seafloor. Then World War II interrupted further analysis.

Around this time, new data from other expeditions also revealed similar ridge features in the Indian Ocean, Arabian Sea, Red Sea and Gulf of Aden. A U.S. Navy expedition had found a large north-south ridge system in the eastern Pacific. While I busied myself with sounding data, Howard was plotting tens of thousands of earthquakes around the world. The pattern we had noticed held. Wherever there was a mid-oceanic ridge, there were earthquakes. When the Indian Ocean earthquake belt was shown to be continuous with the East African Rift Valley, there was but one conclusion: The mountain range with its central valley was more or less a continuous feature across the face of the Earth. Doc and Bruce announced our findings in 1956 at a meeting of the American Geophysical Union in Toronto.

The reaction in the scientific community ranged from amazement to skepticism to scorn. In 1957 Bruce gave a talk on the mid-ocean rift system at Princeton, bringing along a globe we made that showed how the rift system extended all around the world. After the talk, the eminent Princeton geologist Harry Hess, who later developed the theory of seafloor spreading, stood up and said, "Young man, you have shaken the foundations of geology!" The discovery of the mid-ocean ridge system was a revelation, but nobody could explain how it got there.

Bruce believed the rift was a tensional crack caused by the splitting of the Earth's crust. He still did not believe in continental drift. It was very hard to go in the direction of that theory when the boss, Doc, like nearly everyone else in the scientific world, was violently opposed to drift. I was so busy making maps I let them argue. I figured I'd show them a picture of where the rift valley was and where it pulled apart.

There's truth to the old clichés that a picture is worth a thousand words and that seeing is believing. Like most scientists, Jacques Cousteau at first didn't believe in the rift valley. He crossed the Atlantic Ocean in the *Calypso*, towing a movie camera on a sled near the seafloor. They came to where our rift valley was and found it. He took beautiful movies of big black cliffs in blue water, which he showed at the first International Ocean Congress in New York in 1959. It helped a lot of people believe in our rift valley.

In 1956, we first published the North Atlantic physiographic map as an accompaniment to the Bell Telephone System's *Technical Journal*. It was done in pen and ink. The Geological Society of America reprinted the map in 1959. To make the map, we first plotted lines of soundings taken by ships tracking across the ocean. Then we converted the sounding lines into two-dimensional profiles of the seafloor. Then we made three-dimensional sketches based on the profiles and plotted them along the ship tracks. Finally we sketched in areas with no soundings by extrapolating trends observed in profiles made by



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actual soundings. In other words, we made educated guesses to fill in the dataless gaps. Like the cartographers of old, we put a large legend in the space where we had no data. I also wanted to include mermaids and shipwrecks, but Bruce would have none of it.

We continued on, from one sounding to the next, and one ocean to the next. We weren't daunted by the tens of thousands of soundings we had to plot. We were daunted more by all the data we didn't have. For the map of the South Atlantic, in some places we only had spot soundings from the General Bathymetric Chart of the World (GEBCO) series. We used data from the *Meteor* expedition to sketch in the mid-ocean ridge crest and the rift valley. Data from the *Vema* 9 cruise helped us in equatorial areas. We'd use any data available and change our minds as we got more. For example, we at first thought the rift in the Atlantic was a long valley. Then, in the South Atlantic, it was a long valley with some wiggles. Finally, we recognized the fracture zones, which offset the ridge by hundreds of miles.

One of the more challenging areas of the South Atlantic was the remote Scotia Sea, for which there were little or no data available. Fortunately, the pattern of the Caribbean and Scotia seafloors is strikingly similar, allowing us to make a valid extrapolation. The South Atlantic diagram was published in 1961.

We had planned to study the Mediterranean Sea next, but we were diverted instead to the Indian Ocean, because a diagram of it was urgently needed to help plan the International Indian Ocean Expedition. Now our efforts were thwarted by a long-lasting falling-out between Bruce and Doc. There are two sides to that story, but the result was that Doc banned Bruce from Lamont ships and denied Bruce access to Lamont data. He tried unsuccessfully to fire Bruce, who had a tenured faculty position at Columbia, but he did fire me. From then on, I was paid through research grants that Bruce received from the Navy, and I continued the mapping working at home.

Doc could have scuttled our mapping efforts, but Bruce had forged relationships with researchers all over the

world, going to sea on ships from other institutions. By the early 1960s, we had recognized fracture zones in the Atlantic, but we couldn't confirm their general direction and trends until 1968, when Bruce and I were able to secure a cruise aboard the Navy vessel *Kane*. We zigzagged over what became known as the Kane Fracture.

Bruce found alternative sources of data. His book with Charley Hollister, *The Face of the Deep*, had been translated into Russian, and perhaps inspired cooperation from Russian scientists, even during the height of the Cold War. We received extensive soundings from the Soviet ships *Ob* and *Vityaz*, which surveyed the Indian Ocean. Japanese soundings between Capetown and Antarctica, and data from the United Kingdom, Australia and South Africa, and several American oceanographic institutions, were all incorporated into the Indian Ocean map, published in 1964—a truly international effort. And, I should note, it contained a big error. I got so overwhelmed with the fracture zones in the Indian Ocean, I didn't initially recognize a triple junction, where three mid-ocean ridges intersected. We published the map with that error, but corrected it later when new data revealed it.

Inspired by the International Indian Ocean Expedition, the National Geographic Society wanted to commission a map of the Indian Ocean to illustrate an article on it. Some time earlier, National Geographic had received a letter from a little girl in Austria who wrote, "I've been looking at your maps and my father can paint better than you can." Intrigued, National Geographic editors sent their chief topographer to Innsbruck, Austria, to meet the girl's artist father, Heinrich Berann.

Berann did serious paintings in the style of Leonardo da Vinci often with religious themes which, in my opinion, ranks him as one of the foremost painters of our century. But he couldn't earn a living doing this. So he began to paint realistic alpine panoramas for advertisements promoting skiing for tourists. National Geographic commissioned him to paint the Indian Ocean floor and hired Bruce and me as consultants. We loved working with

Heinrich, and his familiarity with painting the Alps translated beautifully to the seafloor. The three of us published a panorama of the Indian Ocean in 1967 and then continued with the rest of the world's ocean floors. The final map we produced for National Geographic was of the Antarctic ocean floor in 1975.

The next step was obvious: to paint a panorama of the entire world's ocean floor. In 1973 the three of us submitted a proposal for the project to the Office of Naval Research. To accomplish it, we had to simplify some of our previous work to accommodate the smaller scale called for by a world map. At the same time we had to update our work to include the vast volume of data that had accumulated over the years.

We'd use all the data we had, but the data didn't provide complete coverage, so there still were blank areas. That was the biggest challenge: providing data for the blank areas. Over the next three years, we traveled back and forth to Austria. I'd go home, work up a blank area with any data we could get, come back to Austria, and Heinrich would paint that area. Constantly adding new data, we changed our minds quite a bit as the panorama took shape.

Our efforts were aided by the advance of technology over the twenty-five years since we first started mapping. In 1962, the World-Wide Standardized Seismic Network (which Lamont helped to establish, with instruments Ewing, Frank Press and other Lamont scientists invented) allowed seismologists to map earthquakes much more precisely. The positions of seafloor spreading centers were more accurately located by magnetic data, the bulk of which was collected by Lamont ships. Ironically, by this time, Ewing had moved to the University of Texas, so we could now use Lamont data, long denied to us, to finalize our maps.

The first proofs for the world ocean floor map arrived from the printers in time for Bruce to take them with him aboard the Navy's nuclear submarine *NR-1* on an expedition to explore the mid-ocean ridge off Iceland. In twenty-five years, the study of earth science had advanced so much, the traditional mountain-climbing geologist with a rock hammer

could now sample the seafloor in a submersible. But Bruce died of a heart attack on that cruise, just a few months before the *World Ocean Floor* panorama was published in 1977.

I think our maps contributed to a revolution in geological thinking, which in some ways compares to the Copernican revolution. Scientists and the general public got their first relatively realistic image of a vast part of the planet that they could never see. The maps received wide coverage and were widely circulated. They brought the theory of continental drift within the realm of rational speculation. You could see the worldwide mid-ocean ridge and you could see that it coincided with earthquakes. The borders of the plates took shape, leading rapidly to the more comprehensive theory of plate tectonics.

I worked in the background for most of my career as a scientist, but I have absolutely no resentments. I thought I was lucky to have a job that was so interesting. Establishing the rift valley and the mid-ocean ridge that went all the way around the world for 40,000 miles—that was something important. You could only do that once. You can't find anything bigger than that, at least on this planet.







*"We were venturing forth to find out how the world worked. We were scientific rather than geographic explorers, but we had some of the same spirit from that earlier era."*





## ICE CAPADES

*Exploring the Arctic Ocean Aboard a Drifting Floe*

by Kenneth Hunkins

I arrived at Lamont in May of 1957 amid heaps of crates and a frenzy of packing. Time was short. At the top of the world, the sun had returned after the long, cold, dark Arctic winter. The ice was already beginning to melt. In less than a month, planes would no longer be able to land on a runway that had been created on an ice floe, a few miles long and about ten feet thick, somewhere in the vast Arctic Ocean. That's where I was headed.

I was twenty-eight, a second-year graduate student at Stanford University, having returned to school after serving as an infantry lieutenant in the Korean War. I had a degree in physics from Yale, but I didn't have the interest, nor probably the talent, to study nuclear physics. I grew up in the winter resort town of Lake Placid, New York, and enjoyed skiing and the outdoors. I didn't want to work only at a desk or in a laboratory. So after completing my doctoral thesis, I expected I would join an oil company to search for oil as an exploration geophysicist.

But by 1957, spearheaded by institutions such as Lamont, the whole concept of geophysics had begun to broaden. The idea of applying classical physics to figuring out how the Earth works had great appeal to me. And 1957-58 happened to be the International Geophysical Year (IGY)—the extraordinary coordinated worldwide effort involving more than 60,000 scientists from sixty-six nations to collect data on the Earth from pole to pole.

As publicity about the IGY began to appear, I was attracted by the planned Sno-cat expeditions to explore the

Antarctic ice sheet with seismic soundings. I wrote to my Stanford professor, George Thompson, who was then on a sabbatical leave at Lamont, to ask how one could join such an expedition. I didn't know exactly where this would lead, but it seemed like it would provide an opportunity for a good thesis topic. He replied that the Antarctic project had been taken to the University of Wisconsin with Charlie Bentley. But there was an opening at Lamont on an expedition involving geophysics from a camp on a drifting ice floe in the Arctic Ocean.

Lamont was eight years old then and beginning to get a reputation for its exploration of the ocean floor with seismic, gravity and magnetic methods using the research vessel *Vema*. A recent article about Lamont in *Scientific American* magazine by the late George Gray had caught my eye with its photograph of the entrance to Lamont Hall. It looked as though it would be an ideal place to work when one was not off on expeditions.

And the project itself sounded like the kind of science that I had trained and yearned for. It would be done with the latest instruments and techniques in a part of the Earth whose perennial ice cover had made it inaccessible and unexplored. While most people who worked on the *Vema* were happy to put into Tahiti or the Azores, the ice and cold were my natural elements—not to mention the element of adventure.

Like many boys, I was brought up reading exploration stories. But by 1957 the age of geographic exploration was over.



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The North and South poles had been reached. Mount Everest had been climbed. It wasn't enough to say that people had stood at a place in the world. A new age of exploration was beginning, though. We were venturing forth to find out how the world worked. We were scientific rather than geographic explorers, but we had some of the same spirit from that earlier era.

Just a few weeks after I had arrived at Lamont, Maurie Davidson and I flew to Fairbanks, Alaska, and then caught one of the last flights to the ice floe camp, Station Alpha, before the ice runway became unusable as summer melting pockmarked it with holes.

42 | Creating Station Alpha had been a bootstrap operation. Reconnaissance flights had started looking for a possible site in March, and in April a C-47 plane equipped with skis landed for a closer inspection of an ice floe due north of Point Barrow, Alaska, at a latitude above 80 degrees. On April 23, a large C-124 plane parachuted equipment onto the ice. A small crew landed on the ice in a smaller ski plane and began to use the airdropped equipment, which included a bulldozer, to fix up a 5,000-foot runway that was level, hard and smooth enough to land dozens of C-54 Globemasters carrying 500 tons of supplies, mostly fuel.

The U.S. Air Force funded our research and supplied logistics with aircraft from Alaskan bases. This was the era of the Cold War, and the prevailing military strategy was based on manned bombers taking the shortest, fastest route across the north polar region to the Soviet Union. That required the Air Force to gain as much information as possible about the Arctic Ocean. But the government interpreted that mission broadly, allowing scientists the freedom to pursue a wide range of basic research. Otherwise I probably would have gone out and searched for oil.

Instead, I was at Station Alpha, where I stayed with the rest of the scientific and station crew until fall, cut off from the world except for radio and infrequent airdrops of supplies. We were on our own in setting up our equipment.

Our living quarters were World War II-vintage arched wooden huts built on pedestals, which provided shade so that

the ice underneath didn't melt as readily. The floorboards were made from the boxes the huts had been packed in. Between the arches was insulating material. At the North Pole, there are only two days in the year, the vernal and autumnal equinoxes, when the sun appears on the horizon. At 80° N, the sun doesn't appear at all until February, creating a twilight that just goes along the horizon for hours on end. But by June 21, the summer solstice, the sun is overhead and it never sets, so you have twenty-four hours of daylight.

When people first got to Station Alpha, they often were so charged up, they worked around the clock. But after a few days of staying up all day, they got exhausted, and got back to a diurnal cycle, paced by regular meals at the mess hall.

It was a very uniform environment, white and flat, and it didn't change much. We were packed in amid other floes, with open water between us, and the whole pack slowly drifted with the wind—something we learned—a few miles per day. During the summer, the sun melted the ice, forming rivulets and then great pools of water, making a mess of everything. We spent a lot of time wading with our boots in ice water, which we tried to drain by melting holes in the ice. Summer temperatures averaged thirty-two degrees, though they got as cold as forty to fifty degrees below zero for crews that overwintered. We had special parkas, but no heaters. I had spent a winter in the front lines in Korea, but I'm not tough or rugged. There are tougher people than I. It's more how you react to it, a matter of mental acclimatization.

There were medics aboard, but no doctors. If you had a major accident or even appendicitis, it would do you in. One fellow on another ice camp had heart problems and died. And we lost an Air Force man, who in the darkness and blowing snow, accidentally walked near an airplane propeller. In probably its last tick, it hit him in the head.

Still, it was easy compared to the war, and the kind of people who undertook the mission accepted the risks.

For a scientist, it was heaven. Taking the newest scientific equipment into one of the least-known parts of the

world ocean was almost guaranteed to produce some noteworthy and exciting new results. Only two drifting stations from the U.S.S.R. and one from the U.S. had preceded us, and they had drifted in quite different parts of this fourth-largest of the world's oceans. We could always think of new experiments, and we had the opportunity and a lot of time to do them. There were few distractions, besides a few movies and the 6,000-calorie-per-day meals provided by Air Force dietitians (just keeping warm uses up lots of calories). Mail came via parachute through a military APO box number, but it never went out. So we put in long days. It was a lot of work to keep the whole camp running, to say nothing of our instruments and the scientific program.

There were about twenty-five people on the station with scientific parties from other organizations, including the U.S. Weather Bureau and the University of Washington, as well as Air Force support. I benefited greatly from working closely with Norbert Untersteiner, our first station leader, who recently retired as chairman of the Atmospheric Sciences Department at the University of Washington. While others collected a range of data on temperatures, winds, weather, sea ice and marine biology, I focused on all the geophysics research led by Lamont. Daily we worked out our position from sun angles with a theodolite as we drifted slowly northward. We were fortunate to be able to use models of the same instruments developed for exploring the open ocean from the *Vema*. The precision depth recorder (PDR), Thorndike deep-sea camera and Ewing piston corer we used were all designed and built at Lamont. We took continuous records of ocean depths, sub-bottom layers, magnetic and gravity fields along our drift track. We also sampled the ocean waters and bottom sediments and photographed the floor of the ocean with instruments lowered on cable through a hole cut in the ice and down through the two-mile depths of water.

The PDR, developed by the electronics team working under Doc Ewing's guidance, provided ocean floor profiles with a precision of about one meter. Back at Lamont I spent a lot of time learning to interpret these records with the

help of Bruce Heezen. This instrument revolutionized our understanding of that seventy percent of our planet covered by ocean water. Instead of scattered spot soundings, we could get continuous profiles showing topographical details of exposed rock on the ocean bottom or the smooth cover of sediments. Particularly striking were the featureless abyssal plains, the flattest parts of the solid Earth with depth changes of only a few meters over hundreds of kilometers.

One of my jobs was laying out an array of seismometers on the ice much the same as that used for oil exploration on land, except that our ice floe was drifting over ocean depths of 10,000 feet. Twice a day, I set off explosions with dynamite, creating seismic waves that reflected off the seafloor and were recorded by the seismometers.

For weeks on Station Alpha, we had been drifting over an abyssal plain flatter than a pool table. Then one night I went to bed, woke up the next morning, set off my dynamite and wham! the instruments showed that the ocean depth had decreased enormously. We had found on the bottom of the ocean a previously unknown mountain range as tall and wide and rugged as the Rockies. The discovery of the Alpha Cordillera, named after the station, was another of many that Lamont scientists were making that literally changed the face of the Earth and that would help revolutionize our understanding of how the Earth's continents and oceans were created.

We also took magnetic and gravity measurements to find out how deep the ocean sediments were and what was beneath them. We took sediments cores, which got pretty well used up over the years because so many scientists wanted to study these hard-to-get samples. We brought back the first photographs of the floor of the Arctic Ocean. The photos showed that the bottom life was less abundant than in the Atlantic at similar depths, but they also showed a curious collection of rocks scattered on the seafloor. Our dredges hauled up samples of them and we saw they were striated and scratched—all features of rocks dragged by glaciers on land.

Later I spent three weeks in Queens University, Belfast, Northern Ireland, studying these dredged rocks with Walter



Schwarzacher, one of our Alpha ice mates. We established that the rocks had originated from land, most probably from the northern part of the Canadian Arctic Archipelago. Evidently the rocks had been frozen in the ice of advancing glaciers, which had rafted offshore and later melted, dropping the rocks to the ocean bottom. Nobody had had any idea that such a process happened. Decades later and even today, ice rafting has proved to be an essential clue to unraveling important details about Earth's changing climate history.

Our dredges of mud from the ocean floor also contained mollusc shells that were later studied by Arthur Clarke at the National Museum of Canada. One mollusc, a large spiral cone, was identified as a new species, *Colus hunkinsi*, which Clarke named after me.

We measured the ocean currents flowing below the ice and were able to detect, for the first time ever, a spiral pattern, the Ekman spiral, which had been theoretically predicted a half-century earlier. In 1893 the Arctic explorer Fridtjof Nansen of Norway observed that sea ice always moved off to the right of the wind. Aboard Station Alpha, we, too, could see it happening day by day. Nansen figured out that the phenomenon was caused by the Earth's rotation. In turn, the ice acts on the water beneath, which moves a little more to the right. That upper layer of water causes the layer of water below it to move to its right. Think of a deck of cards fanned out into a spiral. This had been too subtle and hard to detect in the open ocean, but we could actually measure it from the stable platform of the ice floe.

So it went on until late August when the runway froze solidly enough to permit airplane landings again. In the fall I was relieved by Frans van der Hoeven, but the following spring I was back on the ice. Shortly after my return, we were reminded just how precarious Station Alpha was.

Ice floes drifting in a pack often collide, with enormous force, creating cracks and building pressure ridges that could split ice floes and engulf buildings atop them. Clamped in a vise between other floes, ice often would be pushed upward, forming tall ridges that would march ahead and consume ice floes. In April 1958 a crack opened instantaneously at Station

Alpha, right through our runway. The Air Force wanted to evacuate immediately. But Untersteiner wanted to try to keep the station going. He worked the radio, because the big guys in Anchorage didn't want a disaster, and he prevailed upon the Air Force commanders to evacuate all but a skeleton crew of ten.

We decided to move the whole camp about a mile away to another floe. It was an enormous job. We had to move everything over the ice. We manhandled stuff over the ridges. We used bulldozers to build roads through ridges, some of which reached ten feet high, and dragged everything through. Everyone was so tired. We worked until midnight and finally everyone was exhausted and went to bed. In the morning we saw that two Air Force men had managed to move the mess hall during the night. That was essential, the turning point: There was a place to go inside and have a cup of coffee. We said to ourselves, "O.K., we can do this." Through the whole ordeal, we had managed to maintain our scientific measurements, and Station Alpha continued on.

At least until the darkness of the following November, that is, when the ice floe cracked again, this time so badly that it had to be evacuated in a dramatic rescue. Several days of bad weather and the polar darkness made it difficult for rescue flights to locate Station Alpha for several days, and the airplanes had to land skillfully on a runway cut short by the crack. When George Cvijanovich, our last man on the station, returned to New York, he appeared on the Ed Sullivan TV show to describe the evacuation, and we went into the TV studio in Manhattan to see the show.

Three months before the final breakup, another event marked a sea change in the Arctic manned research program. We knew something was happening when an Air Force radioman began acting secretly. On a little station like Station Alpha, it's hard to keep things hush-hush. Then our camp commander, an Air Force major, launched a little boat with an outboard motor into a lead between two floes. He just kept going up and down the lead. We thought the isolated camp life had finally gotten to him.

The next morning, the impressive conning tower of the *USS Skate*, the nuclear-powered submarine, emerged. The major had been making noise for the *Skate*'s sonar to home in on. The crew came onto the ice, overwhelming our community. They stayed a half-day and left precipitously, as the ice seemed to be moving in and threatening to pinch them.

Shortly after, America's strategic policy switched from manned bombers to guided missiles fired either from land-based silos or, more important, nuclear submarines. Now the Navy needed more information on ice-covered Arctic waters, and our Arctic research program made a smooth transition from Air Force to the Office of Naval Research (ONR), which had been funding a major part of the *Vema*'s oceanographic research. The ONR also took a broad interpretation of its mandate to support Navy research and led federal support of basic research in the post-World War II years, both before the National Science Foundation was organized and during its early days.

Oceanographic research from drifting ice continued for many more years with Lamonters leading the geophysical investigations. There was Station Charlie in 1959, again on an ice floe of frozen sea water, and then, from 1962 to 1974 there was T-3, an ice island rather than an ice floe. T-3 was a massive piece of freshwater ice, eight by fourteen kilometers in extent and fifty meters thick, which had broken from the Ellesmere Ice Shelf and had drifted along with the ice floes composing the thinner pack ice.

A number of new instruments were introduced on T-3 almost as soon as they were in use on research vessels in open oceans. These included the nuclear precession magnetometer and the Satellite Navigation System (SATNAV), which preceded the present Global Positioning System (GPS). The SATNAV system was a classified secret at the time but Joe Worzel, Lamont's associate director, managed to get permission from the Navy to build four units at Lamont from plans supplied by Johns Hopkins Applied Physics Laboratory, which had developed the system. We took one unit to T-3, one went on the *Vema*, and one on the *Robert D. Conrad*, and the

fourth stayed at Lamont as a spare. We now had nearly continuous positions of T-3 as it drifted and no longer needed to make sun and star shots while standing at the theodolite outside in the numbing cold.

There was also Lamont's Bill Donn's microvario-barograph, which measured sensitive air pressure variations. With it, we recorded at T-3 the last Soviet nuclear blasts in the atmosphere from the Arctic Ocean island of Novaya Zemlya before the Test Ban Treaty of 1963. Though we never heard the explosions because their frequency was so low, the blasts drove the pen of the instrument off scale and Novaya Zemlya didn't seem so far away from us at that time.

We accumulated a wealth of data on the Arctic Ocean's seafloor, ocean currents, marine life and the complex interactions among the ocean, sea ice and atmosphere—all of which helped build our present picture of the Earth and its climate system. We gave four dimensions to a unique region of the planet that on most maps, and in most people's minds, was a featureless white expanse over a black hole.

The data we collected in the Arctic were pieces of a puzzle that many Lamonters were engaged in putting together. When I wasn't on the ice in the Arctic Ocean, I lived in an apartment on Franklin Street in Piermont, which was home to bachelor Lamonters when they returned from sea. The roster changed frequently but included Bruce Heezen, Marcus Langseth, Jules Hirschman, Manik Talwani and many others, besides occasional visiting oceanographers from other countries.

In this rolling crash pad we enjoyed great camaraderie. It was a heady atmosphere for young scientists who were totally involved in their work. The field was wide open and there were no manuals for what to do. Funding was available if you wanted to work hard; nobody ever worked 9 to 5—it was too exciting. Responsibility had come early in our careers and we had developed fast. Our work was our lives and we talked about it long into the evening, discussing our research, our instruments, how a method was working. Everyone had his own data from different parts of the world and we brought them together and looked to see how they all fit.



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We weren't competitive. We were generous. We worked together. There was so much to do and room for everyone.

I benefited from all the other people at Lamont, and most of us, I think, would have been lesser scientists if we had worked elsewhere. We maximized our potential here. The whole atmosphere energized people and frequently provided opportunities for cross-fertilization of ideas. These characteristics were often directly traceable back to Doc, who was always ready to enter a new field or revisit a thoroughly investigated old field, especially when some new experimental results steered him into it.

For example, at mid-century most seismologists had focused almost exclusively on studying earthquake-generated seismic waves called body waves, which travel through the body of the Earth, to discover the major features of the Earth's interior, including the inner and outer cores and some crustal details. But following these body waves on seismographic recordings was a long train of high-amplitude waves that were largely ignored, except by theoreticians such as Rayleigh, Love and Stoneley. These were surface waves, which traveled around the Earth in the outermost layers without penetrating deeply.

Filed away in Doc's mind was a long-standing interest in surface waves, dating from his student days when he worked for an oil company and observed explosion-generated surface waves that propagated in the shallow water and mud layers of the Louisiana bayous. One of his earliest publications when he was a professor at Lehigh describes surface waves generated by small explosions that traveled through the ice cover of a lake near the university. Those previously overlooked surface waves proved to be a valuable, untapped tool for studying the Earth and a hot topic in seismology in the 1950s and 1960s. Wenceslas Jardetzky, Frank Press and Doc co-authored the definitive scholarly monograph on the subject.

As Doc's involvement with oceanographic research and the *Vema* increasingly occupied his time, seismology came under the leadership of one of his earliest students, Jack Oliver, and research on surface waves expanded to almost a small industry. Some of the earlier seismology

students under Jack were Jim Dorman, Mark Landisman, Lynn Sykes and Gary Latham.

Here is where I entered with my seismic data from the Arctic Ocean. The records contained information not only on sub-seafloor layers from body waves, but also on surface waves traveling through sea ice. Jack offered me space in the newly built seismology building, and I enjoyed the stimulation of his active group there.

I gradually developed my material into a thesis acceptable to my Stanford professors while working partly at Lamont and partly at Stanford. Jack flew to California to attend my transcontinental thesis defense, and after it was over—and years after my exciting beginning aboard Station Alpha—I felt fortunate to join the world of geophysicists finally as a full-fledged researcher. I stayed at Lamont for my whole career. The opportunities and atmosphere to do research that we enjoyed at Lamont would have been difficult to duplicate anywhere else.



*"Over the past four decades since I became a graduate student at Lamont, seismology has come full circle, finally fulfilling its long-thwarted promise to verify a nuclear test ban."*





## EARTH-SHAKING EVENTS

*Seismology, Plate Tectonics and the Quest for a Comprehensive Nuclear Test Ban Treaty**by Lynn R. Sykes*

My phone rang, waking me from a sound sleep at 5 a.m., well before the age of answering machines that today would allow me to take a call at a more decent time. Only a few hours before, I had returned home to New York City from three days of canoeing in the Adirondacks during the Memorial Day weekend of 1974. The caller was Dr. Eric Willis of the Department of Defense, who asked me to join a U.S. team that was leaving that evening for Moscow to negotiate what was to be known as the Threshold Test Ban Treaty, or TTBT. Struggling to wake up and to think clearly, I said yes. Eric thanked me and said I needed to get down to Washington to pick up my visa before the Soviet Embassy closed at noon. In the rush to get there in time, I hadn't had time to get together my clothes, scientific materials and money, so I returned to New York, hurriedly packed, phoned a few people to let them know what I was doing and headed back to Washington on the Eastern Shuttle. I had only the tail number of the plane that was leaving from Andrews Air Force Base with our delegation. The Shuttle pilot had radioed ahead that I was coming, and the plane to Moscow waited for my slightly late and breathless arrival at Andrews.

I had no idea if our sojourn in Moscow would last a few days or weeks. We were informed that our purpose was to explore whether the Russians were sincere about negotiating a bilateral treaty with the United States to limit the size of underground explosions for nuclear weapons tests. In an effort to counter the ongoing political storm

over Watergate, Henry Kissinger, President Richard Nixon's national security advisor, had advised the U.S. government to seek a relatively quick and modest arms control treaty with the Soviets. Negotiating a more complex and comprehensive treaty, such as a Strategic Arms Limitation Treaty (SALT) to limit long-range delivery systems for nuclear weapons, would have been a much more ambitious and lengthy endeavor.

Our delegation included representatives from the Department of Energy, which funds laboratories that produce and test nuclear weapons, the departments of Defense and State, as well as the Arms Control and Disarmament Agency, the CIA and the Joint Chiefs of Staff. The delegation was headed by the U.S. ambassador to the U.S.S.R., Walter Stoessel, though a member of Kissinger's staff made many of the important decisions after communicating with Kissinger and a committee called the "back-stopping" panel, a similar group of experts on nuclear testing in Washington. Our delegation was unusual in that it also contained two university scientists, Dr. Eugene Herrin from Southern Methodist University in Dallas and me. Our expertise lay in interpreting signals from seismic waves that are set in motion by earth-shaking events such as earthquakes or underground nuclear explosions. These waves travel through the Earth and over its surface and are recorded by seismometers positioned all around the globe. As it turned out, Herrin and I came to form a buffer between two warring factions within the U.S. government—



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those who sought a verifiable treaty and those who opposed it as an unadvised step toward a complete ban on nuclear testing.

After two weeks, when it became clear that the Soviet Union was indeed serious about negotiating a TTBT, several U.S. agencies replaced their representatives with higher-level officials. In fact, some agencies in Washington wanted to replace Herrin and me with others from within the government. Ambassador Stoessel resisted on the grounds that we made significant contributions to the technical discussions and to the balance of the delegation. About two weeks into the negotiations the ambassador asked each of us on the delegation to state whether the allowed explosive yields of nuclear tests should be set at a threshold of 100 or 150 kilotons, numbers conveyed to him from Washington. Herrin and I each stated that seismic waves generated by explosions of either yield could be readily detected all over the world and their seismic signals could be distinguished easily from those generated by earthquakes.

The final details of the TTBT were negotiated by Kissinger in the first few days of July 1974. The treaty, which set the testing threshold at the more conservative 150-kiloton level, was signed a few days later by Nixon and Soviet Premier Leonid Brezhnev. En route home, I flew on a U.S. Air Force transport plane that had delivered a red convertible to Moscow, Nixon's gift to Brezhnev.

In 1974 I thought the TTBT was another step toward a Comprehensive Test Ban Treaty (CTBT) that would outlaw all nuclear weapons tests. I had no idea that 22 years would pass before a CTBT finally would be approved overwhelmingly by the United Nations and then signed by the president and leaders of 148 other nations, including states that acknowledged having nuclear weapons. In fact, the TTBT was signed about midway between the adoption of the CTBT in 1996 and first serious calls for a CTBT in the 1950s—soon after the U.S. and the U.S.S.R. had tested large hydrogen (thermonuclear) bombs in the atmosphere.

In 1958, after a frenzy of mushroom clouds bursting in air, a group of experts from Eastern and Western countries gathered in Geneva to discuss ways to verify a treaty to halt all nuclear testing. At their first meeting they had much information on nuclear explosions in the atmosphere but data on only one very small underground nuclear explosion, code-named RAINIER, which was conducted by the U.S. in 1957.

Concurrently, 1957-58 also marked the International Geophysical Year (IGY), the ambitious coordinated multinational scientific campaign to study the Earth. For the IGY, Lamont had deployed about a dozen seismograph stations around the world with new instruments developed by Frank Press and Maurice Ewing. At one of those stations at Lamont, Jack Oliver observed seismic waves generated by the largest underground nuclear explosion conducted by the U.S. in 1958, the nineteen-kiloton BLANCA test. The U.S. government had quickly recognized seismology's potential for detecting and identifying underground nuclear tests, and a panel of technical experts recommended in 1957 that the U.S. should greatly expand funding of seismology to increase fundamental understanding and to develop better instrumentation. That funding program, called Vela Uniform, almost instantaneously transformed seismology from a sleepy, poorly supported scientific backwater to a field flooded with new funds, professionals, students—and excitement.

The modest seismograph network established by Lamont during IGY became the prototype for the World-Wide Standardized Seismograph Network (WWSSN), installed in 1963 with funds from the Vela program. The WWSSN comprised about 125 stations in many countries around the globe, each with identical Ewing-Press type seismometers. Before the WWSSN, those wishing to study just a few earthquakes or explosions would have to write many letters to the operators of stations around the world asking for copies of their seismograms. Obtaining a sufficient number of records often would take more than a year. Even worse, the recordings came from a great variety

of instruments, many of which were poorly calibrated. For scientists, using the resulting data was often like comparing a sparse sampling of apples and oranges, some of which were rotten. The WWSSN changed that forever, providing a wealth of available, well-calibrated data from an expanded range of sources. By essentially "viewing" the seismic repercussions of an event from many angles and directions around the planet, seismologists for the first time could construct a more three-dimensional and detailed picture of the event. That was especially true for seismologists at Lamont, which was one of the few places that had acquired and usefully arranged microfilm copies of records from all of the WWSSN stations for the entire twenty-year period of their operation.

Seismology and its instruments for recording earthquakes became the main technology for detecting, identifying and locating underground tests. And the stakes were raised after the U.S. conducted additional explosions underground in 1958. Department of Defense officials expressed doubts about the technical experts' predictions, arguing that seismic signals from underground explosions were both smaller and harder to distinguish from the signals of earthquakes of comparable size. In addition Edward Teller, often called the father of the H-bomb, and Albert Latter of the Rand Corporation argued that seismic signals from explosions could be greatly reduced in size, and effectively muffled, by detonating them in large underground cavities. They convinced many people in the late 1950s that the ability to evade detection or disguise nuclear explosions would keep ahead of the science of monitoring nuclear tests.

I came to Columbia and Lamont in September 1960 just as funding from the Vela program commenced, and my years as a graduate student from 1960 to 1965 were a frightening time during the nuclear arms race. Intercontinental ballistic missiles were being tested and deployed. The U.S.S.R. and the U.S. carried out many large nuclear explosions in the atmosphere in the late 1950s and early 1960s. The Russians detonated the largest nuclear explosion ever

conducted at their Arctic test site in 1961. It had a yield of about fifty megatons (50,000 kilotons), 4,000 times the size of the Hiroshima bomb of 1945. The world came the closest it has come to nuclear war during the Cuban missile crisis of 1962.

I shared an office in Lamont Hall with three other scientists, including Paul Pomeroy, who was several years ahead of me as a graduate student. For his Ph.D. thesis, Pomeroy was examining seismograms of nuclear explosions recorded at Palisades and the IGY stations. In the early 1960s Lamont and Cal Tech each competed to discover methods for distinguishing the seismic signals of underground nuclear explosions from those of earthquakes. In 1963 Pomeroy and Lamont graduate student Robert Liebermann discovered a method called Ms-mb that was to become one of the most trustworthy discrimination techniques. Ms was a measure of long-period surface waves, which travel around the Earth's circumference; mb was a measure of short-period P waves, which arrive before surface waves because they take a shorter route through the body of the Earth. The method takes advantage of the differing nature of the two types of seismic sources: Underground nuclear explosions instantaneously crush a relatively small area of surrounding rock, which absorbs the force of the blast and generates seismic waves that emanate outward in all directions. In contrast, earthquakes are caused by a slip of one block of rock along a fault, a process that generates seismic waves over relatively longer periods of time, from greater areas and in asymmetrical directions.

Unfortunately, extensive testing of this technique was completed just a few years too late for the test ban negotiations of 1963. At that time the U.S. government concluded that effective identification of underground explosions was too difficult. Hence, they were excluded from the Limited Test Ban Treaty, which outlawed explosions in the atmosphere, space and underwater. While the LTBT led to a great decrease in radioactive pollution of the atmosphere, it did not halt, or even slow down, the nuclear arms race, which continued via underground



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testing and the development of new delivery vehicles for nuclear weapons.

The early 1960s also marked an era in which the frontiers of seismology expanded rapidly. A wealth of new data poured in from the IGY network. When I arrived at Lamont, records were just coming in of the great Chilean earthquake of May 1960, the largest shock of the previous 100 years. Those data—along with records from a new strainmeter installed by Lamont just days before the earthquake in a deep abandoned mine at Ogdensburg, New Jersey, and from instruments operated by Cal Tech and UCLA—provided unequivocal evidence that the quake had caused the entire planet to vibrate as a unit, like a ringing bell. The data also generated excitement that reverberated in Lamont Hall, where another of my office mates, Lee Alsop, a new post-doc, was on the forefront of studying this new discovery of the existence of Earth's free oscillations, one of the greatest breakthroughs in geophysics.

Jack Oliver, my faculty advisor, urged me to dive into the piles of IGY records to see what exciting phenomena they would reveal. For my Ph.D. thesis I decided to use the data from those stations to analyze how seismic surface waves propagated across oceanic areas. But to accomplish this task, it was essential to have much more accurate data than were available at the time on the locations and origin times of earthquakes in oceanic areas. At the time, calculating earthquake locations by hand—synthesizing latitudes and longitudes, seismic wave arrival times and depth data from more than a hundred stations—was a laborious, time-consuming, mistake-prone prospect. By necessity, people tried to shortcut the otherwise overwhelming process, extracting general information using only a few readings and marking off approximate distances. That, of course, led to locations that could easily be off by hundreds of kilometers, especially in remote oceanic regions.

I was aware that Bruce Bolt, a post-doctoral scientist at Lamont, had just finished writing perhaps the first

computer program to locate earthquakes more precisely, though he hadn't tested it extensively before he moved on to do other research at Berkeley. I revised Bolt's program, creating a computer library—on a four-inch-thick stack of old-fashioned computer punch cards—of all the locations of all the world's seismograph stations. Each station was designated by a three-letter code. For a given earthquake, I could enter the arrival times and depths of seismic waves for a series of three-letter codes. That made it possible to process 25 to 50 earthquakes in one computer run, to do in minutes what would have taken a person days. Using an IBM 7090 computer at the NASA Goddard Institute for Space Studies near Columbia, which was then the world's largest and fastest computer, I began amassing a dataset of more precisely located earthquakes that had occurred over several recent decades, largely along the mid-Atlantic ocean ridge system and in the Southern Hemisphere.

The influx of data from the IGY stations and the availability of computers combined to make this possible. But another important ingredient was simply deciding to work on the problem—because there are always ten times as many things that you might think of working on as you can do. A legacy of both Ewing and Oliver, which they conveyed to many students, is that it is important to pick a topic that is ripe for some really important research. You don't have to be an absolute genius to make a very important discovery. A lot of very smart people work on second- and third-rate problems.

I discovered that many of the calculated locations of earthquakes in the southern oceans were in error by as much as 500 kilometers (300 miles). My revised, more precise locations were far less scattered around the ocean floor. Instead they aligned in an intriguing zig zag pattern. The earthquakes coalesced along the centerlines of mid-oceanic ridges but then suddenly changed direction by nearly 90 degrees, zagging for a certain distance and then just as abruptly zigging perpendicularly again to resume their original direction. In the far southeastern Pacific

near 55°S, I identified a large “zag” that offset two parallel segments of the mid-oceanic ridge by about 500 kilometers.

In 1963 I speculated in a paper that a great fracture zone intersected the ridge system at that location. At that time, fracture zones had been identified only in the northern and equatorial Pacific as long lines of rough topography that intersected the East Pacific Rise. The discoverer of these great fracture zones, William (Bill) Menard of Scripps Institution of Oceanography, was also a reviewer of my paper, and he had some bathymetric data that supported my hypothesis. But after seeing my data Ewing quickly arranged to survey the area with the *Eltanin*, a research vessel that worked in Antarctic waters for the National Science Foundation. The survey confirmed the existence of one of the world’s greatest fracture zones, thereafter named the Eltanin Fracture Zone. I went on in the next two years to identify similar zigzag-shaped earthquake patterns in the Arabian and Norwegian seas.

The discovery of the Eltanin Fracture Zone was a classic example of the cross-pollination that was, and still is, a hallmark of Lamont. My thesis work on surface waves in oceanic regions couldn’t help being complemented and influenced by the vast amounts of a variety of geophysical data that Lamont was collecting from most of the oceanic areas of the world. I kept abreast of the discoveries that Bruce Heezen, Charles Drake, Maurice and John Ewing, Jack Nafe, Manik Talwani and other Lamont scientists were making at a feverish pace about the mid-oceanic ridge system, deep-sea trenches and the deep structure of the solid Earth beneath oceanic areas. And at the same, what I was discovering contributed to solving the geological mysteries they were investigating.

I came to realize that identifying great features on the seafloor was of greater importance than my thesis work on surface waves. So for the next few years I turned my efforts to detailed mapping of earthquakes along mid-oceanic ridges and island arcs. I made a major study of hundreds of shocks in the southwestern and northwestern Pacific near trenches and island arcs. In these regions,

earthquakes were known to occur at depths as great as 700 kilometers. I found that the earthquake zone was not widely scattered but rather swept downward in a remarkably narrow arc from the seafloor to deep into the Earth. This line of research led me in 1965 to join Bryan Isacks, who received his Ph.D. from Columbia about the same time I did, in studying earthquakes in the Tonga-Fiji region in the southwest Pacific, where deep earthquakes occurred frequently.

While I was in Fiji, Jim Dorman of Lamont wrote to me about a paper published in *Nature* by J. Tuzo Wilson, who had used my zigzag patterns to propose a radical theory on seafloor faulting. At first glance, the zigzag pattern could reasonably be explained by a mid-ocean ridge breaking into two parallel segments that slid apart in opposite directions along a perpendicular fault, or fracture zone—like the first step on a Nordic Track. Wilson, however, proposed that the movement along the fault was exactly opposite and counterintuitive: the motion of the blocks along the fault was *toward*, rather than away from, each other—like the second step on a Nordic Track. The reason, Wilson explained, was that the ridge segments weren’t moving apart; instead they were being separated by new seafloor that was created at the ridges and spreading outward and between them.

Wilson called the faults between the two ridge segments “transform” faults because instead of continuing past the ridge, the fault would be transformed into the ridge itself, in a continuing step-like fashion. If he were right, blocks of seafloor would move against each other along the transform faults to cause earthquakes. But that energy would then jag 90 degrees along the ridge, so that there should be no earthquakes along the fracture zone beyond the ridges.

Like most people at the time, I was skeptical about Wilson’s hypothesis. It advocated two concepts that had many doubters: seafloor spreading and continental drift. The idea began to simmer in my mind that I could prove or (more likely, I thought) *disprove* Wilson by using my more



precise earthquake location data, as well as focal mechanisms obtained for those shocks from the new seismic network. (Focal mechanisms are diagrams of fault motion at an earthquake's focus, or hypocenter, the site of the initial rupture, where strain energy is converted into elastic wave energy.) But when I returned to Palisades in November 1965, I was more focused on finishing a manuscript on my Fiji-Tonga research on earthquakes and the deep structure of island arcs.

Then in February 1966 Jim Heirtzler at Lamont called Jack Oliver to say he and Walter Pitman had some exciting new magnetic evidence. I went along with Jack and they proceeded to show us what became known as the "magic profile" across the East Pacific Rise. Magnetic data on rocks on either side of the Rise were symmetrical down to the smallest detail out to 500 kilometers. The mirror images of either side of the mid-ocean ridge provided a compelling argument that new material created along the ridge crests had been magnetically imprinted and then had spread outward in either direction. I knew right then that I had to begin working immediately on Wilson's transform fault hypothesis. I started the next morning.

Within a few months I obtained about fifteen earthquake mechanisms along the ridge system and its extension into East Africa using microfilm records from the new WWSSN. Very quickly I knew I had something really big, and that I was not going to prove Wilson wrong. I was going to prove him right.

My results showed that shocks along the active parts of fracture zones involved lateral motions along strike-slip faults in the direction predicted by Wilson and opposite to that predicted for simple offset of the ridge system. Earthquakes along ridge crests themselves involved normal faulting, as both sides of the ridge spread apart. Along the fracture zones beyond the ridges, there was a notable absence of earthquakes. My data strongly supported the proposed processes of seafloor spreading, transform faulting and continental drift.

Word of my work soon spread at Lamont. Paul Gast, a Lamont geochemist, invited me to present my work at a

symposium in 1966 at the Goddard Institute for Space Studies. My work and that of Pitman and Heirtzler and others at Lamont on magnetism and heat flow made a great impression on the invited audience, which included many of the greatest contributors to the plate tectonics revolution: Edward Bullard, Xavier LePichon, Dan McKenzie, Bill Menard and Fred Vine. No one had intended the meeting to be a definitive conference on earth mobility, but it turned out to be the turning point for those concepts in the United States.

Soon after, Oliver and Isacks interpreted their newly collected data in the deep Tonga-Fiji seismic zone as evidence that one segment of oceanic crust was plunging down and thrusting under another—a process that was soon named subduction. In 1967 McKenzie and Robert Parker and, independently, Jason Morgan each outlined models for the relative movement on the surface of a sphere of large undeformed spherical caps comprising the outer 100 kilometers of the Earth—the tectonic plates. Their model—which included the concepts of continental drift, seafloor spreading, subduction and transform faulting—soon became known as the plate tectonic hypothesis. LePichon, then at Lamont, calculated the relative movements of a number of large plates, including the then-unknown directions and rates of motion at subduction zones. Once we learned about his results, Isacks, Oliver and I decided to write a comprehensive paper, *Seismology and the New Global Tectonics*, which in 1968 pulled together information from many seismological studies that related to the plate tectonic hypothesis. Soon after, most geophysicists in North America became converts, and in a stunning scientific revolution, our understanding of our planet was rapidly and radically transformed.

Plate tectonic theory provided a new foundation for understanding why most earthquakes, volcanoes and young mountain belts occur where they do. At the same time, the theory helped us understand and predict earthquake mechanisms in certain regions of the U.S.S.R. and China, allowing for better discrimination between quakes and blasts. It provided a way to calculate earthquake

depths, making it possible to determine, for example, that a small earthquake is deeper than ten kilometers and hence cannot be a nuclear explosion. It furnished an understanding of why seismic waves propagate efficiently in the hard rock of the Soviet and Indian test sites, but not in the powdery alluvium of the U.S. test site in Nevada—and thus why explosions of the same yield in Russia and India generated waves with much greater seismic magnitude than they did in Nevada. Failure to appreciate, or to acknowledge, that fact led the United States government to accuse the Soviet Union until the 1990s of cheating on the Threshold Test Ban Treaty that I had helped negotiate in 1974.

Throughout those years, many people in the departments of Defense and Energy ignored or did not believe seismological evidence that took these geological differences into account. They insisted (wrongly, as it turned out) that using surface seismic waves would not produce more accurate calculations of yields. Throughout those years, I and other colleagues fought hard to give voice to the contrary evidence. In the late 1970s, for example, Carter Administration officials made plans to test bombs exceeding the TTBT limit in answer to the Soviets' alleged cheating—based on what I believed was a biased report. I wrote to Frank Press, the former Lamont seismologist who was then Carter's science advisor, who convened another panel that explored the evidence again. I would like to believe that my raising either hell or enough valid scientific points did help forestall the U.S. from setting off explosions above the TTBT limit and stirring up a hornet's nest in Russia at a time of tense relations between the two countries.

My participation in the TTBT negotiations, on numerous scientific advisory committees and in testimony before Congress on five occasions, convinced me that until 1992, the notion that nuclear testing was vital to national security was the main obstacle to agreement by the U.S. and other nuclear powers on a complete and verifiable halt to nuclear testing. Many people in the agencies responsible for developing and testing nuclear weapons had long argued that

seismological verification techniques were not sufficiently reliable, which—conveniently, I would argue—allowed them to continue testing and developing new nuclear weapons.

In the late 1980s, Paul Richards, a Lamont seismologist who has also devoted years to the nuclear arms control effort, and I were among twenty or so experts on an advisory panel that provided information to Congress' Office of Technology and Assessment. In 1988, the OTA conducted the first major independent review on seismic verification of nuclear testing. For the first time, the departments of Defense and Energy were not the sole sources of information. In 1988 the differences between the Russian and U.S. test sites were finally confirmed by Joint Verification Experiments in which Americans and Russians each were allowed to monitor tests within meters of the shotpoints at each other's test sites. In 1992, for the first time, Congress passed legislation limiting nuclear weapons testing. In 1996 the Comprehensive Test Ban Treaty was signed, with an extensive international monitoring system set up to verify it.

Since the first call for a CTBT in the late 1950s, seismological instrumentation and techniques have improved immensely. Serious evasion schemes, while still topics of scientific and political debate, are considered exceedingly difficult to conduct without detection by seismic methods and satellite imagery. Methods to muffle explosions in underground cavities have been determined to be unfeasible for explosions larger than ten kilotons, somewhat smaller than the yields of the weapons of 1945 and much smaller than those of nuclear weapons deployed on long-range delivery systems today. Over the past four decades since I became a graduate student at Lamont, seismology has come full circle, finally fulfilling its long-thwarted promise to verify a nuclear test ban.

Plate tectonics was a nice bonus.

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*I thank Lonny Lippsett for editing the manuscript and my many colleagues at Lamont and elsewhere with whom I have interacted over the past 40 years.*



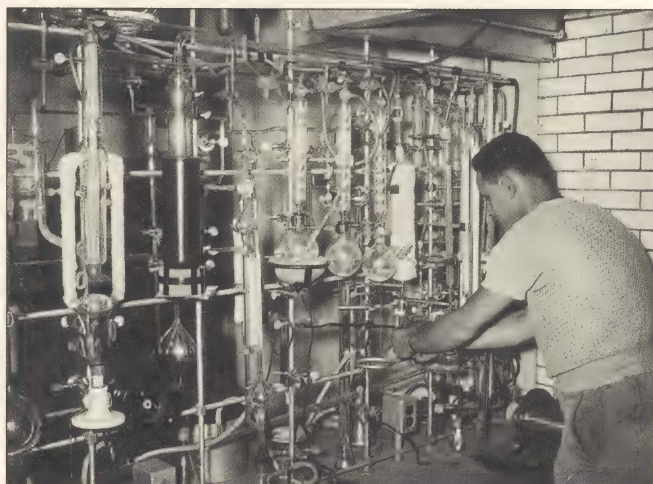




*"We geochemists occupied our own sphere in a  
west wing of the former Lamont mansion.*

*We were a group apart—  
in the mansion, in subject matter . . ."*





# BETTER LIVING THROUGH GEOCHEMISTRY

## *Tracking Chemical Clues to Investigate the Earth*

by Wallace S. Broecker

In the early days, two distinct scientific kingdoms existed at Lamont. The dominant one, of course, was Maurice Ewing's geophysics and marine geology realm. But running strong and independently was the isotope geochemistry program of J. Laurence Kulp.

We geochemists occupied our own sphere in a west wing of the former Lamont mansion—having gravitated naturally to the kitchen, with its essential gas line, running water and drains, and to the basement, where we installed the big lead shields used to house our radiocarbon dating apparatus. We were a group apart—in the mansion, in subject matter, and also because many of us were recruited by Kulp from Wheaton College in Illinois (Billy Graham's alma mater) and shared Kulp's background in fundamentalist Protestant Christianity. Our geophysical counterparts quickly pegged us "the theochemists."

The early 1950s were heady days in the field of geology, which was being revitalized and transformed by an invasion of physicists and chemists with new techniques inspired by wartime research. While Ewing blazed one trail, unleashing modern physics onto the study of the Earth, Kulp just as eagerly rushed to infuse chemistry into geology, opening up wholly new and hitherto unexplored lines of inquiry.

World War II had stalled the promising research started in the 1930s by the patriarchs of isotopic geochemistry: Alfred Nier, Harold Urey, Fritz Houtermans and Harry Thode. With the war's end, geochemistry sprang into high gear, fueled by a surge of eager new students, new funds

and new equipment. In 1952 when I came to Lamont, my eyes bulged as Kulp showed me his burgeoning domain of mass spectrometers, vacuum systems for melting rocks, and low-level counting systems for measuring radioisotopes. I was awed.

I had come to Lamont from Wheaton at the suggestion of Paul Gast, who had been assigned as a "big brother" to me when I was a freshman. Gast had previously had a summer job in Kulp's lab, and he had arranged one for me after my junior year. In September, my wife and I were ready to drive back to Illinois to finish my senior year and Kulp said, "You don't want to go back to Wheaton. They don't have anything for you. Stay here." Two days before classes began at Columbia, Kulp somehow accomplished the transfer. Maybe it helped that during the summer I had stumbled onto a way to fix a faulty wire that had caused the radiocarbon detectors to malfunction. Within months, I was given my own research program involving radiocarbon dating.

Dating was the key to this rapidly blossoming field. At the foundation of geochemical research is the natural radioactivity of certain elements found in rocks and water. "Parent" isotopes of the elements decay into more stable "daughter" isotopes at fixed rates. (Their half-lives are the time required for one-half of a quantity of the parent isotope to decay, forming a stable daughter isotope.) So a specimen's age can be computed by measuring the relative amounts of remaining radioactive parent and its daughter products. Analyzing samples gives you a "when," providing a good start to figuring out a "how."



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Radioactive carbon-14 is especially useful for dating organic matter. It is produced by cosmic rays in the atmosphere, incorporated into carbon dioxide, and then fixed into plant matter by photosynthesis, thus providing all plant and animal material with a built-in clock. But there are lots of different isotopes, each of which allows you to date and trace all sorts of processes that go on throughout the Earth system. By knowing where and how isotopes entered the ocean, it is possible to trace ocean circulation patterns. Isotopes can also be used to study the exchange cycles of gases between the ocean and the atmosphere and thereby to start to unravel the ocean's role in regulating the planet's climate. You can measure isotopes in fossilized coral reefs to deduce the rise and fall of global sea levels and by extension, the volume of water locked in glaciers during ice ages. In more recent days, isotopic measurements of ice cores from polar glaciers have taught us that Earth's climate may be susceptible to abrupt shifts. Geochemists are like detectives identifying strategic locales to find clues, using various chemical pathways to extract telltale information, sorting out causes and effects and reconstructing possible sequences of events—all in an effort to answer an array of questions about the Earth's history, how the Earth system operates and how it will respond, for example, to the atmospheric buildup of greenhouse gases since the onset of the Industrial Revolution.

When we first started out, the field was wide open. There were so many interesting questions ripe for plucking. Potential applications for geochemistry seemed to grow on trees, and so did money. The lab was open twenty-four hours a day, seven days a week, anytime anybody wanted to come in and work. Exciting new results poured in weekly and everyone in our group discussed them spiritedly and freely, all of us equals in a field too young to have developed a hierarchy. Larry, as we called Kulp, gave us a lot of responsibility and opportunity. He was great at raising money and inflating our egos. He flattered us into believing we were indispensable, provided the wherewithal to conduct our research and fueled us youngsters with ideas. It was a scientific Garden of Eden.

While I took on radiocarbon dating, my fellow graduate students were given similar challenges, developing and refining measurement techniques for other isotopes. George Bate, on leave from teaching physics at Wheaton College, built a mass spectrometer for lead isotope measurements. Don Carr did potassium-argon dating. Paul Gast measured strontium isotopes; Bruno Giletti, tritium; Karl Turekian, trace metals; Herb Volchok, radium; Paul Damon, rare gases; Walt Eckelmann, lead isotopes; Wayne Ault, sulfur isotopes.

We were all figuring out the bugs in the new techniques and equipment, so in 1953 when we started to get occasional measurements with spikes of high radioactivity, we first blamed it on faulty electronics and made sure they were working right. That seemed to straighten it out for a while, but then the measurements went crazy again. We thought the system might be contaminated by varying day-to-day levels of radon gas in the air that we were blowing into the system with a women's hair dryer to dry carbon samples. Then it hit us: Could it possibly be that we were picking up traces of fission products wafting over to New York from atomic tests that had recently begun at the Nevada test site? Sure enough, we were. We not only figured out how to get accurate measurements once again, we also got caught up in the Cold War.

In 1949 Kulp had learned about Willard Libby's Nobel Prize-winning discovery of the carbon-14 dating method and offered to be a "slave" in Libby's lab for several months in exchange for learning the technique. He brought it back to Lamont, which was perhaps the first lab to apply it to geology. In the summer of 1953, Libby called a meeting of high-level scientists, including several Nobel Prize winners, to discuss the potential harmful ramifications of radioactivity released to the atmosphere by nuclear weapons tests. Kulp was invited because he was among the few people in the world at that moment who could measure low-level radioactivity, having learned it at Libby's feet.

Libby was a member of the Atomic Energy Commission (AEC), which was meeting in three weeks to make decisions on the problem and desperately needed



good information. Kulp had the temerity to volunteer that the Lamont lab could prove in that time frame that low levels of strontium-90, an isotope produced by atomic bombs, could be measured accurately enough to track nuclear fallout.

We collected samples: shells from the ocean, soil and vegetation from Lamont, a calf bone, some soil and cheese from Wisconsin. We chose Wisconsin partly because it was roughly halfway from the test site to New York, but primarily because somebody's uncle had a farm in Wisconsin and could send us samples quickly. We worked nearly round-the-clock, working out the chemistry, adjusting the equipment, doing the tests, and lo and behold, we could measure the radioactive strontium in all the samples except for shells. Radio-strontium had landed on the soil and had been incorporated into grass, which was eaten by the cow, which produced the milk for the cheese. Eventually humans would eat that cheese, as well as vegetables grown in soil, and concern grew that radioactive strontium would follow the chemical footsteps of calcium into human bone, inducing cancer-causing mutations, especially in young, growing children. Further, strontium retained its radioactivity for 30 years and it remained airborne long enough to be carried over wide distances, making strontium-90, as Libby put it, as ubiquitous as sunshine.

Thus "Project Sunshine" was launched to understand the fate of radio-strontium, radio-caesium and other nuclear fallout isotopes. Over the next decade, nearly 10,000 samples of human bone samples, mostly from cadavers, from countries on all continents, arrived at what came to be known as "Kulp's Kitchen," where they were cremated into ash and tested for strontium-90 content. The study, in many ways, launched a Lamont tradition in geochemistry of tackling global environmental questions: Its objective was to determine how much strontium-90 would wind up in human skeletons in people of different ages, locales and dietary regimes all over the world. The results, published by Kulp and two other Wheaton alumni, Walt Eckelmann and Arthur Schulert, clearly helped persuade politicians to

negotiate the treaty banning atmospheric testing of nuclear weapons.

Since this research was at first classified, we students needed to obtain what was then called Q clearance. This produced some embarrassment: Giletti's mother-in-law was a registered Communist and Chuck Tucek's car had been spotted parked near a Communist picnic. Fortunately, my only connection seemed to be on the plus side: Senator Joe McCarthy's brother delivered gasoline to my pop's service station!

But I did manage to run afoul of the FBI. Early on, Kulp got the idea of measuring krypton-85, another fission product of A-bomb tests. This particular isotope interested him because as a noble gas it remained airborne. So we purchased one liter of krypton gas from Linde's Air Reduction Plant in Buffalo, New York. I substituted it as the filling gas for our radiocarbon counter and measured an average of eight disintegrations of krypton-85 per minute. Kulp was delighted. Based on the published kilotonnage of the three A-bombs exploded to that point, he had predicted very nearly this amount.

The next day, he dashed off to Washington, D.C., and proudly presented this result to the military. Within hours, the FBI pounced on our lab, telling us in no uncertain terms to destroy all records of this measurement and never again mention radio-krypton. Only later did we learn that thousands of such measurements were being made at Argonne National Laboratory outside Chicago. Tony Turkavich, a University of Chicago professor, had realized that by subtracting the amount of krypton-85 released by the United States and Great Britain from the total in the atmosphere, the extent of plutonium production by the Russians could be assessed. Kulp had thought only in terms of A-bombs. Turkavich realized that krypton-85 was also produced by reactors designed to breed plutonium.

In 1953, already busting out of our cramped quarters in Lamont Hall and with the promise of healthy, long-term funding from the AEC, the geochemistry group began to think about having its own laboratory. Designed by us and



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a local architect, built by a local contractor named Bill Hans with the ingenious help of a local heating engineer named Russ Gurnee (a future president of the Explorers Club), we built the Geochemistry Laboratory at the bargain cost of \$125,000—\$4 per square foot. We dedicated the building in October 1954 by inviting participants of a "Crust of the Earth Symposium" at Columbia to a buffet dinner in a large tent nearby. Unfortunately, the night before the event, Hurricane Matilda blew the tent far into the adjacent woods, and the hundred or so guests had to eat sitting on the floors of our new building.

As the 1950s drew to a close, Kulp's original geochemical children dispersed to populate positions at major universities, oil companies and industry and to spawn ever-branching new generations in the ever-widening field. Turekian went to Yale, Gast to Minnesota, Damon to Arizona, Giletti to Brown, Eckelmann to Exxon (then ESSO), and Bate returned to teaching physics at Wheaton's twin, Westmont College in California. I remained at Columbia with an appointment as assistant professor.

In the early 1960s, Kulp and two of the 1950s Ph.D. crop, Herb Volchok and Don Carr, started a business, Isotope Inc. In 1966, Kulp had to make a choice and opted to give up his Columbia professorship for the corporate world. Kulp's departure put the huge burden of managing the large geochemistry program entirely on my young shoulders. During a six-month sabbatical at Caltech in 1965, I was tempted to stay there. My price for returning to Columbia was the hiring of my old friend and mentor Paul Gast as professor. After a tense meeting with Columbia's Provost Jacques Barzun, renowned for his dislike of science and scientists, the deal was struck.

I have been at Lamont ever since, working on scores of problems simultaneously and intermittently. A good scientist keeps many different problems on his or her screen. Many of these, at any given time, will be stalled: Nothing's happening; you've pushed the problem as far as you can. But then you read something, or hear something, or think of something, or somebody makes a new measurement and

wham! You see how you can push that particular problem much further and you work on it until once again it stalls and wait for something else to happen. Something always pops up, so you never run out of interesting and important things to pursue. In the Earth system, all of the problems are interrelated, so an advance in one usually acts as a catalyst for another. My Ph.D. thesis had chapters on how the ocean mixes, climate history, and geochemical cycles—how chemicals move through the Earth system. In ever-greater depth, I've been pursuing those very broad topics ever since.

The 1960s marked the beginning of decades of fruitful collaborations with Taro Takahashi, a Lamont mainstay. We had been fellow graduate students, but he worked on campus in economic geology with Charles Behre and I was a Lamont man, so our paths rarely crossed. In 1958, Taro took a post-doc with Kulp, but we still saw little of each other because he spent almost the entire year on the *Vema* making some of the first large-scale measurements of carbon dioxide (CO<sub>2</sub>) gas concentrations in the ocean.

In 1962, Columbia's John Imbrie organized an expedition to the Bahama Banks, and Taro and I joined his group at a small fishing outpost on Fraser's Hog Key. It proved to be one of the most exciting periods of my life. Our tiny geochemical team (Taro, Ross Horowitz and I) managed to gather an enormous amount of chemical and radiochemical data, which greatly expanded our existing knowledge of the geochemical processes that create calcium carbonate (CaCO<sub>3</sub>) deposits on the broad and shallow Bahama banks. One of our most interesting findings, buried in a now largely forgotten paper, was proving conclusively that the Bahamas' famous whittings, which intermittently streak the otherwise crystal-clear waters, came from stirred-up bottom sediments, rather than from spontaneous chemical precipitation as many thought (and some still think). Through serendipity, we also found that the bottom sediments gave off copious amounts of radon, the end product of a radioactive process that starts with naturally occurring uranium in seawater. Uranium is soluble, but it decays into thorium, which is not, and precipitates into seafloor sediments. Thorium further decays into



radium, which is once again soluble, and radium decays into radon, a gas that over time seeps up from the seafloor. A decade later, we fully exploited this discovery to assess the rate of vertical mixing in the abyssal ocean.

During this period, Taro taught at Rochester University, but he soon moved closer to Lamont, becoming a distinguished professor at Queens College in New York City. Finally, in 1972, he succumbed to his love for research and joined the Lamont staff. Since 1980, he has held major Lamont administrative positions, but he never allowed himself to spend less than half his time on his research projects. Building on the experience gained during his *Vema* days and in the Bahamas, Taro took on the mission of measuring and chronicling carbonate chemistry throughout the world's oceans. His extraordinarily successful career-long venture has contributed enormously to our knowledge of how the ocean and atmosphere exchange CO<sub>2</sub>, the heat-trapping greenhouse gas produced by fossil-fuel burning that has been accumulating ever more rapidly in our atmosphere since the onset of the Industrial Revolution. Understanding how the ocean absorbs and stores excess CO<sub>2</sub> is an essential component to our ability to manage global warming.

The 1960s also marked progress on the climate history front, which I first became interested in as an outgrowth of radiocarbon dating research in my thesis. In 1956, David Ericson and Goesta Wollin, pioneers in Lamont's deep-sea sediment core laboratory, had noticed a boundary in cores that was marked by a sudden reappearance of shells of a warm-water-loving plankton species that upon death sank to the seafloor and were preserved in the sediments. In the classic Lamont tradition of applying all tools available to a problem, I dated those shells at 11,000 years, noting that it marked a critical transition when Earth's climate warmed and the glacial era turned into the warmer present-day Holocene period.

Radiocarbon dating provided many insights like this, but it had a built-in cap: After 40,000 years or so, too little radiocarbon remains to permit reliable dating. So we moved

into high gear to harness the daughter isotopes of uranium, whose longer half-lives promised to extend geochronology further into the past. Working first as students and then as post-docs, Dave Thurber, Archie Kaufman and Richard Ku took advantage of a new innovation, introduced to Lamont by William Sackett, that allowed the alpha particles emitted during the radioactive decay of various uranium and thorium isotopes to be measured separately. The most exciting application was dating corals by measuring the buildup of thorium-230 (with a half-life of 75,000 years) as its parent uranium-238 decayed.

Robley Matthews at Brown University had heard of our work and asked us to determine the ages of corals from a series of raised coral reef terraces on Barbados. Each time Earth's sea level dropped, one reef died and another formed around the new lower shoreline. Sea levels fell and rose as glacial ice sheets waxed and waned, so dating the corals offered clues to the cyclicity of ice-age climates. Robley sent us samples from two reefs and Thurber promptly obtained ages of 82,000 and 124,000 years. I got very excited, for these dates rang a bell.

In 1875, Scottish scientist James Croll first outlined the theory that variations in Earth's orbit influenced climate patterns by changing the Earth's distance from the sun and the angle at which solar radiation reached the planet's surface. Croll discovered that Earth's orbit around the sun oscillates between a more elliptical path and a more circular one in a 100,000 year cycle.

As the Earth revolves around the sun, it is also rotating, and the axis of the rotation is not perpendicular to the orbital plane. For most of the year, except at the vernal and autumnal equinoxes, either the South or the North Pole is tilted toward the sun, creating the seasons. As the Earth revolves around the sun, the equinoxes (and hence Earth's solstices, the times when the Northern and Southern hemispheres are most tilted toward and away from the sun) occur at different places around that elliptical orbit—in a roughly 20,000-year cycle known as the precession of the equinoxes. Today the winter solstice in the



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Northern Hemisphere occurs in December; 11,000 years ago it occurred in June. Croll theorized that when the winter solstice in the Northern Hemisphere coincided with a point at which Earth was at the far end of the elliptical orbit, the result would be exceptionally cold winters.

But there is yet another cycle, because the angle of Earth's tilt also varies over time. When the planet's poles lean farther from the sun, they are exposed to less solar radiation annually. In the 1930s the brilliant Serbian mathematician Milutin Milankovitch calculated solar radiation at various latitudes during various seasons back through time and demonstrated that the North Pole's tilt toward the sun increases and decreases in a 41,000-year cycle.

The Barbados coral dates matched this 41,000-year cycle. I called Matthews and proudly announced this discovery. "But Wally," he said, "there is a third terrace located halfway in between the first two." Soon we had analyzed that one as well and found it to be 105,000 years in age. It then struck me that the 20,000-year precessional cycle was far more important than Milankovitch had realized.

On top of this, Jan van Donk and I had analyzed plankton skeletons in sediment cores and saw evidence for another regular climate pulse. Using the new uranium-thorium methods and oxygen isotope measurements, we found evidence for cycles in which glacial expansions over 100,000 years were abruptly terminated by rapid deglaciations—as happened most recently 11,500 years ago to create our current warmer period between ice ages, or interglacial. That pointed to the importance of the elliptical orbital cycle, but why did the deglaciations occur so quickly, rather than gradually?

These lines of inquiry set me off on what has been an ongoing quest to unravel the effects of all these interacting cycles and to understand the driving forces and mechanisms behind the ice ages and other rapid global climate changes.

However, this quest was put on hold during the 1970s by two new efforts launched by our geochemistry group to investigate other fundamental Earth processes. The first involved studies on freshwater lakes at Canada's

Experimental Lakes Area (ELA). The second (in cooperation with scientists at Scripps Institution of Oceanography and Woods Hole Oceanographic Institution) was the very ambitious fundamental study of the world's ocean: the Geochemical Ocean Sections Study, or GEOSECS.

In the late 1960s, I received a call from Jack Vallentyne at Canada's Fresh Water Institute in Winnipeg. Copious supplies of nitrates and phosphates from runoff and wastewater were entering local lakes, supplying "fertilizer" that spurred plant growth and, as a byproduct, sucked all the oxygen out of the lakes' subsurface waters. This process is called eutrophication. Vallentyne and colleagues wanted to figure out the source of the CO<sub>2</sub> that the plants used for this excessive photosynthesis. Aware of our work on CO<sub>2</sub> exchange between ocean and atmosphere, Jack thought that similar techniques might be applied to lakes.

Thus I was introduced to the ELA research station in western Ontario by Greg Brunskill. Located twenty-five miles from the nearest highway, with access only by crude, often washed-out logging roads, this isolated group of small lakes had been transformed into a unique natural laboratory for "perturbation limnology," where scientists conducted experiments to identify the factors that affected lake systems. The force behind this enterprise was David Schindler, a brilliant and hard-driving young limnologist. He and I immediately hit it off and soon we were combining the best of limnology with the best of geochemistry. A parade of graduate students who did class work at Lamont and field work at ELA followed. Weekend canoeing trips through Canada's isolated back country turned many city slickers into avid outdoor enthusiasts and environmentalists, and the science was exciting. Starting with studies of gas exchange, we quickly graduated to research on the fate of metal pollutants and the impact of acid rain, as well as experiments using radiocarbon as tracers to illuminate the processes that affect the biological productivity of whole lakes.

This collaboration had important impacts on environmental policy. For example, the soap and detergent industry had long succeeded in obscuring findings implicating phosphate in detergent as the major cause of eutrophication



in inland waters. Schindler finally proved his point by installing a plastic curtain that prevented mixing between two parts of a dumbbell-shaped lake. To the upstream half, he added nitrate and sugar (but no phosphate); to the lower half, he added nitrate, sugar and phosphate. Within weeks, the lower half turned to green pea soup, while the upper half retained its blue clarity.

Today, detergents no longer contain phosphate. The graduate students who conducted these experiments now hold key positions in the environmental arena: Steve Emerson and Paul Quay are professors at the University of Washington; Richard Bopp at Rensselaer Polytechnic Institute; Tom Torgerson at the University of Connecticut; Sherie Schiff at Canada's Waterloo University; Peter Bower at Barnard College. Ray Hesslien has taken up the reins as intellectual leader of the ELA effort; Andy Hertzog works for the Australian government; Mike Amdurer is an executive in the environmental industry; and the list goes on.

But while my students worked at ELA, I was sailing the seas, as co-head of GEOSECS. It was the Camelot of my career. Like ELA, this project's origin was also serendipitous. During a visit to Woods Hole Oceanographic Institution (WHOI) in the late 1960s, the great physical oceanographer, Henry Stommel, took me aside. I had been making radiocarbon measurements on dissolved inorganic carbon in seawater in an effort to learn the rate at which water circulated in the deep ocean. Stommel said the research would pay off only if we collected samples at stations along the full length of the Atlantic. I made a quick calculation: Based on twenty-five stations, twenty samples per station and \$1,000 per sample, the program would cost \$500,000 plus ship time. In those days, that was a lot of money. I was stunned.

"But Henry," I asked, "where would we get the money?" He then let me in on a secret. The National Science Foundation was planning a project to be dubbed the International Decade of Ocean Exploration (IDOE). Soon after, Harmon Craig of Scripps and I approached WHOI Director Paul Fye with a plan for what became the GEOSECS program. Fye agreed to smooth the way for us

with the NSF committee empowered to launch IDOE. Soon, this wonderful project was born.

Between its inception in 1969 and its completion a decade later, GEOSECS consumed not a half-million, but \$25 million of NSF's research dollars. It brought together what proved to be an incredibly efficient, unified and high-spirited team of geochemists from most of the world's major oceanographic laboratories. Its goal was to measure the distribution of a wide range of isotopes, elements and compounds throughout the world ocean, using them as tracers to map the deep circulation of the ocean, to measure the rate of this circulation and, along the way, to unveil any other hitherto unknown processes in the ocean that geochemical measurements might reveal.

So we measured not only radiocarbon but a host of other radioisotopes (lead-210, silicon-32, radon-226 and cesium-137), as well as numerous nutrients, trace metals and carbon dioxide. On July 21, 1972, *R/V Knorr* departed from Woods Hole amid fireworks, bands and popping champagne corks. It went to the northernmost reaches of the Atlantic, and over the next ten months, went down the western Atlantic basin and across to the Scotia Sea, then up the eastern basin and returned in May of 1973—sampling all the way.

We didn't stop there. Aboard *R/V Melville* samples were collected at 147 stations from the Bering to the Ross Seas and the Mediterranean and Red Seas. At the end of GEOSECS, everyone concluded that the program wasn't very good for the institution of marriage, since we scientists were at sea so much. But to this day, GEOSECS results still provide the underpinning for virtually all studies of marine chemistry. In particular, the measurements of tritium and radiocarbon proved key to determining how much CO<sub>2</sub> produced by fossil fuel burning was being taken up by the ocean. In an article in *Oceanus* recapping GEOSECS, MIT's John Edmond compared us to the New York Yankees of the 1950s. Everybody hated us, but had to admit we were the best. Clearly, our high-powered group did not lack in arrogance. We knew that we had pulled off a scientific coup.

While we scientists received most of the glory, the



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truth is that GEOSECS was successful largely because of the efforts of one man, a New Zealander named Arnold Bainbridge. Given a very short preparation period and a very large challenge, he organized a crew of top-notch technicians, designed and built a new generation of geochemical instruments (rosette samplers, titrators, autoanalysers, extraction systems), created new sampling techniques and installed shipboard computer programs—all of which were essential for the survey's success. He did this all incredibly well. The entire program went like clockwork, rarely missing a beat.

Meanwhile, as we were working on lakes and oceans, back at Lamont, Gast and his group were exploring hard rocks, starting on a route that would make Gast revered among planetary chemists. Gast conceived the idea that the high pressure existing in Earth's mantle would have its greatest effect on larger ions, making them more likely to be sucked up by magma that is created in the mantle and later carried to the crust. The less melting, the greater the proportion of these so-called lithophile elements, because the magma would be less diluted by other major rock-forming elements.

To get a better grip on this phenomenon, Gast pioneered the measurements of the entire suite of rare earths, a group of elements whose nicely graduated atomic diameters made them perfect candidates for testing the importance of size. Gast was so successful in this venture that when the lunar exploration missions began, NASA asked him to set up a mass spectrometry laboratory in Houston for similar studies of moon rocks. He took a leave of absence from Lamont, planning to be gone for only two years. But he soon learned that he had incurable cancer and decided that it made better sense to spend his remaining days at the Space Flight Center. When Paul died in 1973, Columbia lost a star, and I lost a very special friend. A better big brother no man has ever had.

Gast's death by no means ended Lamont's hard-rock program. Keith O'Nions took up the reins in 1975, remaining at Lamont for five years before being called home as a Royal Society Professor at Cambridge. Alan Zindler followed and

now the position is now held by Steve Goldstein, who earned his Ph.D. at Lamont in 1986.

We made two other stellar catches for our petrology program: David Walker and Charlie Langmuir. Walker designed and built his own laboratory device that can simulate the enormous pressure deep inside the Earth. Through a series of ingenious experiments, he and his students were able to determine which of the minerals that crystallized under high-pressure conditions in the mantle would sink toward the core and which would float toward the surface. He has also used his multi-anvil device to shed light on the mystery of how rocks can slide past each other even under high pressure to generate deep earthquakes. And in a different line of inquiry, the multi-anvil has also provided the high pressure to synthesize new crystals with potential to be superconductors.

Langmuir and his entourage of excellent students became known for their work on volcanism and the processes that create new seafloor at the crest of mid-ocean ridges. They made systematic studies of the relative abundances of the major oxides that make up ridge crest basalts. These long-overlooked small variations in composition proved to be key to the understanding of processes involved in generating magmas beneath the ridges.

While all this was happening, the shell of our original building was evolving. I say "shell" because except for the concrete floor, cinderblock walls and wooden ceiling joists, no original material remains (except for the windows, light fixtures and furnishings in my office). As every university administrator has learned, chemistry and instrument labs must be completely renovated every decade or two. We attached four separate additions one after another onto the rear of our building.

The first of these is infamous. It was designed by Tom Parker, a nineteen-year-old helper in the electronics group. Bob Marnane and his Lamont Building and Grounds crew put its foundation and shell in place. The interior was completed by "us." Everybody in the geochemistry group spent a half-day a week putting up wall-board and ceiling tile. At one point, we got a great scare.

Our administrative assistant Ellen Coxe spotted two young girls climbing on the roof of this unfinished wing and in a stern voice told them to get on home. They scampered off to the adjacent Fox farm and breathlessly told their daddy that some nasty lady had chased them off the roof of the new Lamont building. Daddy said, "What new building?" Big trouble, for daddy was head of the local zoning board and we had no building permit. Only much pleading by our longtime administrator Arnold Finck succeeded in putting aside the resulting stop-work injunction.

The second addition was Skipper Alley, named for our lab cat of seventeen years. The third was the petrology wing, dubbed "The Walker Wart." Finally, in the late 1980s, "Takahashi's Palace" with its seminar room and deck was added. The next step, we all hope, will be to level this unwieldy geochemical slum and replace it with an entirely new building.

In the early 1980s, having pushed the GEOSECS results to the limit, I picked up a dormant strand. Way back in 1954 I was introduced to Pyramid and Mono Lakes in the deserts of the western United States by Phil Orr, curator of archeology at the Santa Barbara Museum of Natural History. Corraling me after a talk I gave on radiocarbon dating in Los Angeles, he told me that while clearly I knew some physics and math, I didn't know a "goldarned thing about the Earth." He was determined to correct this deficiency. And he did, taking me on a geologic tour of the West. In exchange, he sought—and got—lots of free radiocarbon dates. So we both benefited from this symbiotic relationship.

I returned to these closed basins in the desert, enchanted by the challenge of unraveling the history of Lakes Lahontan, Bonneville and Russell. These lakes were ten times larger during the last glacial period than their present-day remnants. To maintain that size, far more rain had to have fallen in the region then than now. What could account for this huge shift? Searching for clues we have analyzed tufas—calcium carbonate rock formations precipitated by algae—which formed at now high-and-dry

former shorelines of the lakes. Like many other glacial puzzles, this one remains to be solved.

I also turned my eye to the chemistry of salty lakes in the region. In particular, I saw a unique opportunity to determine the rate of exchange of  $\text{CO}_2$  between the lakes and the overlying atmosphere. Unlike the vast and wide ocean, lakes offered a closed system, where variables could be controlled more easily. I took advantage of the large spike in the concentration of radiocarbon in atmospheric  $\text{CO}_2$  that resulted from the rash of nuclear bomb tests in the late 1950s and early 1960s. Tracking that spike, I began what turned out to be a thirty-year series of radiocarbon measurements on the inorganic carbon dissolved in the alkaline waters of these lakes—studies that have involved many Lamont students and staff.

The measurements compared well with those in the ocean and in experimental wind tunnels, with one mystifying exception: Mono Lake, whose radiocarbon content had increased nearly ten times faster than expected. In an attempt to understand this strange behavior, graduate student Rik Wanninkhof directly measured the rate at which gases were exchanged between atmosphere and lake. He injected sulfur hexafluoride gas into the lake and then measured the rate at which it escaped to the overlying air. Rachel Oxburgh attempted to reconstruct all the processes that governed the lake's carbon content and found that volcanic  $\text{CO}_2$  was seeping into the lake from below. Rather than solving the enigma, these studies fortified the conclusion that the lake's larger-than-expected post-1950s radiocarbon inventory could not be explained by the influx of radiocarbon from bomb testing alone. We began to suspect that someone, faced with the necessity of clandestinely disposing several curies of artificially produced radiocarbon, had decided to hide it in Mono Lake. It was an excellent choice, because Mono Lake has such a large concentration of dissolved carbon that even the addition of a large amount of radiocarbon led to an increase of only a few percent in the ratio of radiocarbon to carbon in its waters. We may have discovered a near-perfect crime, for no suspects have turned up.



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Lamont also had turned its geochemical attention to another type of water body: rivers. Jim Simpson and his graduate students started research on the Hudson Estuary using a twin pontoon float boat lovingly named *Notpul*, after our electronics engineer Bob Lupton. This craft had a tough start in life. While being towed behind a rented truck loaded with assorted gear along the New York Thruway on the way to Mono Lake, the trailer hitch broke and *Notpul* ended up upside down in a roadside swamp. Thus baptized, however, this handy platform served us for many years. Richard Bopp, Doug Hammond, Bruce Deck, Curtis Olsen and the rest of Jim's graduate students undertook to understand the interacting circulation, sedimentation and chemical processes of a beautiful estuary that was rapidly deteriorating. When it was learned that General Electric had spilled a large quantity of highly toxic polychlorobiphenyls (PCBs) into the upper Hudson, Jim's gang was poised to study its spread toward the sea. The Lamont studies played a large part in the campaign to shut down the input of sewage and other pollutants to the Hudson. Its waters once more have acceptable oxygen levels and its shad and striped bass thrive.

In the post-GEOSCECS era, another discovery caused me to return my attention to paleoclimatology. In 1980 researchers in Switzerland and France simultaneously surprised the world by demonstrating that Earth's atmosphere contained thirty percent less CO<sub>2</sub> during the last glacial period than it did in the present post-glacial Holocene period (at least until the Industrial Revolution). The ocean and atmosphere always strive slowly but surely to achieve an equilibrium in their CO<sub>2</sub> contents, so over time scales of a century or more, the ocean governs the atmosphere's CO<sub>2</sub> content. Hence the roots of this significant change during the glacial epoch must reside in the ocean. In particular, the answer must involve the strength of what is known as its "biological pump." To grow, marine plants remove CO<sub>2</sub> from surface waters via photosynthesis. The debris from these plants and the organisms that feed upon them eventually fall from the sunlit surface into the dark

interior where hungry resident organisms feed on it. This process, which draws down CO<sub>2</sub> from the ocean's surface waters and from the overlying atmosphere, somehow must have been stronger during glacial time. This has been clear from the beginning and the solution seemed within easy reach, but it has eluded my best efforts and those of many other scientists.

In late 1997, an elaboration of an idea pioneered by the late John Martin of the Moss Landing Marine Laboratory hit me: Perhaps, as Martin proposed, ocean ecosystems were fertilized by iron contained in the abundance of tiny soil and rock particles, collectively called "dust," that became airborne during drier, windier glacial times. But the link isn't direct. The secret lay in the fact that iron fosters the ability of organisms to convert atmospheric nitrogen (N<sub>2</sub>) into forms they can use (NH<sub>3</sub> and HNO<sub>3</sub>). With Gideon Henderson, now an Oxford don, I outlined this theory in a paper that has stimulated considerable controversy. Unlike organisms in Schindler's ELA lakes, could oceanic organisms flourish in response to an increase in nitrates alone, without phosphates?

Then came another discovery by the same Swiss researchers who found decreased CO<sub>2</sub> levels during the last ice age. They surprised us with more detailed CO<sub>2</sub> measurements on glacial-age ice cored from the southern part of the Greenland ice cap. Their results suggested that CO<sub>2</sub> levels in the atmosphere underwent sudden jumps, plunging to 200 parts per million during epochs of intense cold lasting roughly 1,000 years and rising suddenly back to 240 ppm during millennia of intermediate cold. Ultimately these CO<sub>2</sub> jumps were shown to be artifacts of an interaction with dust-borne calcium carbonate and not true indicators of past atmospheric CO<sub>2</sub> levels. But before that was determined, I labored to understand what might cause such rapid CO<sub>2</sub> fluctuations. That led me to the concept of the "Great Ocean Conveyor." While the CO<sub>2</sub> results were misleading, the concept proved to be correct.

Today, the driving force of the Conveyor is the cold, salty waters of the northern Atlantic Ocean. Cold and salty



water is denser than warm, less salty water and hence, it sinks to the ocean bottom, propelling water through the world's oceans like a hand pushing downward in a bathtub. The undersea current created in this way is more than sixteen times greater than the combined flow of all the world's rivers. It flows southward all the way to the southern tip of Africa, where it joins a watery raceway that circles Antarctica. Here the Conveyor is recharged by cold, salty waters created by the formation of sea ice, which leaves salt behind when it freezes. This renewed sinking shoves waters back northward, where they gradually warm again and rise to the surface in the Pacific and Indian Oceans.

In the equatorial Indian Ocean, surface waters are too warm to sink. Northern Pacific waters are cold, but contain far too little salt to sink into the abyss. This is primarily because prevailing winds that whip around the planet hit the great mountains of the western United States and Canada and drop their moisture. The snow and rain eventually run down rivers back to the Pacific, adding a dose of fresh water that dilutes the Pacific's saltiness.

North Atlantic waters have only about two percent more salt than Pacific waters, but that is just enough to exceed the threshold that allows them to sink. But if the North Atlantic waters warmed by a degree or two, or if they were diluted by just a bit more fresh water from melting glaciers, sea ice or more rainfall, for example, the threshold would not be achieved, and the waters would cease sinking. Perhaps this thermohaline circulation (from the Latin words for "heat" and "salt") is quite sensitive to even small changes, I thought. The entire Conveyor might turn on or off—with dramatic impacts on global climate.

Today, the Conveyor comes full circle, eventually propelling warm surface waters, including the Gulf Stream, back into the North Atlantic. In winter months, this warm water transfers its heat to the cold overlying air masses that come off frigid Canada and Greenland. Thus tempered, the eastward-moving air masses make northern Europe noticeably warmer in winter than comparable latitudes in North America. Without it, Dublin today would

be as cold as Spitsbergen thirty degrees to the north in the Barents Sea.

The realization that reorganizations of the ocean's thermohaline circulation could force the atmosphere into quite different modes of operation had strong repercussions for the research agendas in laboratories around the world, ours included. Thomas Stocker, who now leads the climate research program in Bern, Switzerland, spent three years in our geochemistry laboratory working with a simplified model to learn the circumstances that might induce the thermohaline circulation to undergo reorganizations. Fritz Zaucker, a Ph.D. student at Heidelberg University in Germany, joined us to conduct his Ph.D. research on the transport of water vapor from the Atlantic to the Pacific, which drives the Conveyor by leaving behind excess salt. Robert Anderson, who joined Lamont in the early 1980s, has targeted the Southern Ocean, using his isotopic and chemical tool kit to revolutionize our understanding of changing conditions in that vast ocean during different climate regimes over Earth's history. His research adds essential new pieces to the puzzle, showing the roles that the Southern Hemisphere plays in global climate change and global ocean circulation.

If the Conveyor changed during glacial times, perhaps it could again today as a result of the ongoing rise in greenhouse gases. Peter Schlosser, recruited from Germany to our group in the late 1980s, has played a prominent role in exploring this possibility. With the help from the Keck Foundation, he established mass spectrometry laboratories for the measurement of helium-3 and tritium and for the measurement of noble gases in yet another type of water body: aquifers. Ralf Weppernig from Bern aided him in the former endeavor, and Martin Stute from Heidelberg, in the latter.

Schlosser and his instruments have had a tremendous influence on Lamont's research program in geochemistry. Much of his research has been concentrated in northern waters. By tracking the varying distributions of tritium and its decay product helium-3, his team showed that the normal sinking of cold, salty waters in the Greenland Sea, which was healthy during the 1970s, came pretty much to



a halt around 1980, and since then has remained shut down. His patient monitoring of the Arctic, which began after his arrival at Lamont, is now bearing fruit. He is demonstrating clear changes in the distribution of fresh water in Arctic surface waters and in the salinity and temperature of subsurface waters, which have implications for the Earth's climate system. Schlosser's research captured the interest of then-Senator Albert Gore, who asked in his book *Earth in the Balance*: "Do these changes in the North herald a response to the ongoing global warming?"

The late 1980s also brought a major discovery by Rick Fairbanks. With Bruno Hamlin and Edouard Bard, both post-docs from France, Fairbanks pulled off a fantastic coup by recovering pristine glacial-age fossilized corals in an innovative drilling operation he mounted in the shallow waters off the west coast of Barbados. These well-preserved coral skeletons contained chemical fingerprints of the seawater at the time they formed. Using uranium-thorium dating, the team calculated high-precision ages of specimens from 9,000 to 18,000 years old. Because the corals grow only in the sunlit zone near the sea surface, the research produced a robust history of sea-level rises through the Earth's most recent transition out of an ice age.

The work also provided another essential tool to refine our high-precision dating techniques. Radiocarbon levels are not constant in the atmosphere. Radiocarbon is produced by collisions between atmospheric nitrogen and cosmic rays. Because the incoming cosmic rays are partly shielded by the magnetic fields of the Earth and sun, and because the strength of these fields varies, the amount of radiocarbon produced and incorporated into atmospheric carbon dioxide changes over time. Thus the radiocarbon clock is imperfect.

Previously, to calibrate radiocarbon dates, scientists have correlated them with tree-ring records whose annual rings can pin down ages precisely. The rub has been the Younger Dryas, a millennium of climate change that interrupted the ongoing deglaciation, sending the Earth into a last frigid fit before the warmer Holocene finally took hold about 11,500 years ago. The Younger Dryas killed off the trees, but Fairbanks' coral

record extended not only through the Younger Dryas but all the way to the time when deglaciation commenced.

Hamlin and Bard precisely dated Fairbanks' corals via the uranium-thorium method. The results stunned us. The uranium-thorium dates showed that radiocarbon dates had increasingly larger errors as one went back in time, reaching 2,500 years at 17,000 years ago. But because of their work, the imperfect radiocarbon clock can now be corrected.

With the 1990s came Biosphere 2, the marvelous glass, steel and concrete structure in Arizona that encloses 3.15 acres of various ecosystems, including a desert, savanna, rain forest and ocean. Jack Corliss, a geochemical consultant for the infamous cult that built and occupied Biosphere 2 in an attempt to explore the feasibility of space colonies, asked if I would meet with John Allen, the enterprise's guru. Over dinner at New York's Santa Fe restaurant, John bewailed the gradual disappearance of oxygen from a sealed system that was supposed to be self-sustaining.

I suggested that the soil inside was probably too rich in organic matter, which hungry microbes were consuming along with the oxygen. But why was there no correspondingly large increase in carbon dioxide, which the microbes would produce as the end product of this consumption? That stumped me.

Soon, graduate student Jeff Severinghaus and I were out in Arizona trying to solve another geochemical mystery. We explored all sorts of avenues. Then Severinghaus' father pointed us to the answer to the riddle—concrete. The excess carbon dioxide was being sucked up by the large amount of concrete in the structure, reacting with calcium hydroxide in the concrete to form calcium carbonate.

About the time we finally got this answer, Ed Bass, who had bankrolled this \$200 million enterprise, decided to pull the plug on the spendthrift cult. They were banished and the interim management asked me to help create a new research thrust there. Thus began Columbia's partnership with Biosphere 2, which has spawned a new earth science education program for undergraduates and some fledgling research. Using monies provided to Columbia by Bass to support the Biosphere 2 effort, we added one more professor

and a new dimension to our research program. Kevin Griffin, who refers to himself as an ecophysiologicalist, is interested in how plants respond to excess atmospheric carbon dioxide from fossil fuel burning and to the fixed nitrogen released to the atmosphere from fertilizer and internal combustion engines.

Griffin is not the only new blood brought into the department in the 1990s. Martin Stute, who came with Schlosser and is now a professor at Barnard College, launched a series of experiments using the "noble gas thermometer" to take the temperature of glacial-age groundwater in aquifers from all the world's continents (except, of course, frozen Antarctica). The technique is based on the principle that the amount of noble gases dissolved in groundwater depends on the temperature of the ground: the colder the air, the colder the ground, the more noble gases dissolved in the underlying aquifer. When rainwater has penetrated some 100 feet into the ground, it has no further contact with the atmosphere, and because noble gases are chemically inert, their concentrations do not change once they become entombed in the aquifer.

The noble gas thermometer gave us a means to measure temperature changes that occurred over land areas during glacial times, to augment our ability to measure past ocean temperatures. Stute's findings that soil temperatures in Brazil and Vietnam were a whopping 5°C cooler than now surprised us because it contradicted previous theories that the tropics did not cool drastically during the ice ages, and it has provoked an ongoing reassessment of how Earth's climate system may work.

In recent years, Pierre Biscaye, a member of Lamont's research staff since 1967, has also pioneered a new geochemical tool to explore climate change, which has gathered an enormous momentum. He and colleagues have identified telltale isotopic fingerprints in fine-grained particles captured in glacial ice cores. Because soils from different areas of the globe have different isotopic characteristics, he and colleagues can track the particles back to their source. Measuring the ratios of neodymium, strontium and lead isotopes, Biscaye first used these fingerprints to pin down that South America's

Patagonian Desert was the source of dust reaching the Antarctic ice cap during glacial times. More recently, he demonstrated that the major source of glacial-age dust in Greenland ice was Asia's Gobi Desert. These studies provide a linchpin for climate modelers reconstructing past atmospheric circulation patterns and conditions in an effort to unravel Earth's complex climate system.

In a similar way, Sidney Hemming, who joined us in 1994 as a post-doc and later became a faculty member, analyzed isotopes of lead and argon in mineral grains of rocks that had been frozen into the bases of advancing glaciers, carried out to sea, deposited on the ocean floor and buried by subsequent sediments. She showed that the debris originated in the Hudson Bay region of Canada and had been dumped into the ocean by huge iceberg armadas (triggered periodically by a mechanism that scientists are still debating). The icebergs surged from the Laurentide Ice Sheet and across the North Atlantic and all the way to the shores of the British Isles and France before completely melting.

Sidney's husband, Gary, also joined Lamont. With Abhijit Sanyal, he analyzed isotopes of trace amounts of the element boron in preserved zooplankton shells, using them as indicators of the ocean's past acidity.

Tanzhuo Liu from China joined us to continue research on rock varnish that he began at Arizona State University. This manganese-rich varnish is found on rocks in all desert regions, thickening at the incredibly slow rate of a few microns per thousand years. The chemical composition of newly formed varnish is strongly dependent on climate, so Liu's electron probe analyses of thin sections of varnish provide a surprisingly detailed record of the wetness of desert regions.

So fifty years after J. Laurence Kulp established the geochemistry group, it thrives with a continuing infusion of bright young men and women. Their ideas have always been given equal status with those of more senior people, and in that spirit of cooperation and excitement, first engendered by Kulp, we continue to uncover fascinating and important new clues that further our understanding of how our planet works.



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Before closing this chapter, I must confess that the credit for our accomplishments has never been properly shared. Over the years, we have been blessed by the presence of a skilled and dedicated support group. The list of people who have contributed so greatly to the morale of our team as well as to its product is a very long one. But a few stand out as truly exceptional. Ross Horowitz, Guy Mathieu, Millie Klas, John Goddard, Mary Lou Zickl, Adele Hanley, and Marty Fleischer would certainly appear at the top of most lists as people who were always willing to go the extra step to ensure

that measurements were made correctly and on time. Ellen Cox stands out as administrator superior. For years, with minimal help and no computer, Ellen handled an immense amount of detail, never missing a beat. Although Ellen's record is hard to match, our current support group of Patty Catanzaro, Debbie Criscione, Moanna St. Claire, Kathy Tumik, Carol Guilfoyle and Charlene Straub matches any group of their predecessors in ability, stamina and willingness. They handle with a smile the ever more burdensome load of paperwork required by federal agencies that support our research.

## SCIENTISTS

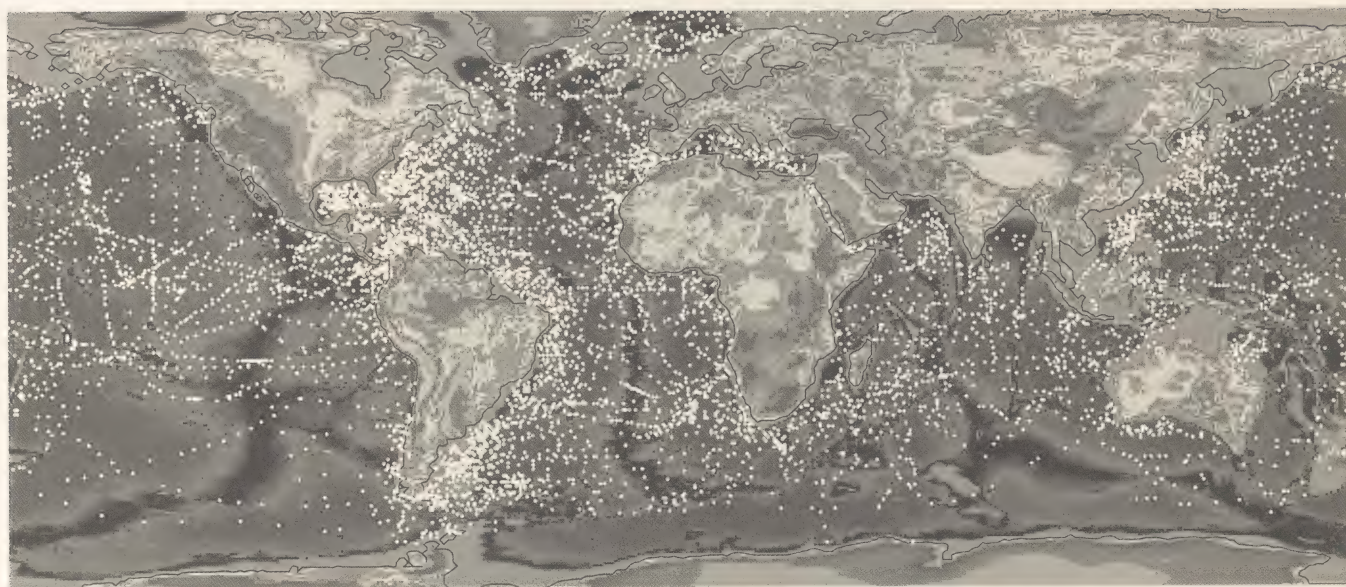
1949-1965 J. Laurence Kulp	1969-1975 Robert Kay	1976-1983 Peter Santschi	1980-1994 G. Alan Zindler	1989-present Peter Schlosser
1957-present Wallace Broecker	1970-present H. James Simpson	1977-present Taro Takahashi	1981-present Robert Anderson	1994-present Sidney Hemming
1962-1964 William Sackett	1970-present Hannes Brueckner	1977-1980 Scott Weaver	1982-1990 James Ledwell	1996-present Steve Goldstein
1955-1961 Arthur Schuler	1973-1979 William Ian Ridley	1977-1993 James Bishop	1982-present David Walker	1997-present Kevin Griffin
1965-1969 Donald Garlick	1974-present Enrico Bonatti	1979-present Charles Langmuir	1983-1992 James Rubenstone	
1965-1970 Paul Gast	1975-1979 Keith O'Nions	1979-present William Smetbie	1988-present John Longhi	
1967-present Pierre Biscaye	1975-1987 Yuan-Hui Li	1980-1992 Hubertus Staudigel	1989-present Martin Stute	

## JUNIOR SCIENTISTS

1956-1958 Allan Walton	1979-1990 Richard Bopp	1986-1992 Richard Wanninkhof	1992-1993 Frank Sirocko	1995-present Steven Chillrud
1957-1959 Taro Takahashi	1980-1987 Bruce Deck	1986-1994 Bruno Hamelin	1992-1993 Kenneth Farley	1995-present Tanzhuo Liu
1957-1958 Paul Gast	1980-1982 Mary Jean Richardson	1987-1994 Edouard Bard	1992-1993 Yaoling Niu	1996-1997 Rachel Oxburgh
1964-1966 Aaron Kaufman	1981-1987 Peter Bower	1987-1994 Scott Stine	1992-1994 Dana Desonie	1996-present Abhijit Sanyal
1966-1967 Jack Dymond	1982-1994 Anne Le Huray	1988-1989 Susan Trumbore	1992-1997 Ralf Weppernig	1996-present Burke Hales
1970-1984 James Lawrence	1983-1985 James White	1989-1991 Jonathan England	1993-1995 Todd Sowers	1996-1998 Clara Castro
1970-1974 Jan Van Donk	1984-1985 Samuel Mukasa	1989-1992 Laurie Reisberg	1993-1996 Rosamond Kinzler	1996-present Joseph Ortiz
1973-1980 Tsung-Hung Peng	1984-1988 David Christie	1989-1995 Vincent Salters	1993-1998 Bai-Hao Chen	1997-1998 Norbert Frank
1974-1979 Norman Evensen	1984-1994 Charles Lesber	1990-1995 Marie Johnson	1994-1995 Franco Marcantonio	1997-present Paul Asimow
1975-1977 Patrick Jos. Hamilton	1985-1986 Francis Grousset	1990-1997 David Archer	1994-1997 Andrea Ludin	1997-present Roland Hohmann
1977-1986 Wilford Gardner	1986-1988 Aldo Shemes	1991-1992 Albert Leger	1994-present Gary Hemming	1998-present Jess Adkins
1978-1981 Paul Hamlyn	1986-1988 Glenn Shen	1991-1995 Deborah Colodner	1994-1998 Gideon Henderson	1998-present Paul Tomascak
1979-1980 Curtis Olsen	1986-1989 Monique Seyler	1991-1995 Yong Lao	1995-present Connie Class	1998-present Yan Zheng
1979-1981 Raymond Hesslein	1986-1990 Cynthia Evans	1991-1996 Thomas Stocker	1995-1998 Manfred Mensch	1999-present Richard Zeebe

## GEOCHEMISTRY GRADUATE STUDENTS

1947-1952 Heinrich Holland	1961-1967 Thomas Anderson	1972-1979 Richard Bopp	1983-1988 Emily Klein	1991-1998 Yan Zheng
1949-1955 Karl Turkian	1962-1966 George Clark	1972-1980 Bruce Deck	1983-1988 Youxue Zhang	1992-1995 Jeffrey Severinghaus
1949-1955 Herbert Volchok	1962-1967 Yuan Hui Li	1973-1978 Jorge Sarmiento	1983-1990 Jeffrey Rosenbaum	1992-1996 Abhijit Sanyal
1949-1956 Donald Carr	1963-1969 Abdalla Abdel-Monem	1973-1978 David Kadko	1983-1989 Jeffrey Ryan	1992-1999 Samar Khatiwala
1950-1957 Wayne Ault	1964-1968 Benjamin Powell	1974-1987 Steve Carson	1983-1990 Kye-Hun Park	1993-present David Ho
1951-1955 George Bate	1964-1970 Jan Van Donk	1974-1980 Robert Cook	1984-1988 Carl Agee	1993-1999 Randy Rutberg
1951-1956 Herbert Feely	1965-1969 Virginia Oversby	1975-1982 J. Robert Toggweiler	1984-1991 Yong Lao	1993-1999 Sean Higgins
1951-1956 Walter Eckelmann	1965-1970 H. James Simpson	1975-1983 James White	1985-1994 Daniel Miller	1993-1999 Stephany Rubin
1952-1957 Bruno Giletti	1965-1970 Michael Bender	1975-1980 Peter Bower	1985-1991 John Crusius	1993-1997 Stephanie Shapiro
1952-1957 Paul Gast	1965-1970 Robert Kay	1975-1982 Peter Michael	1985-1992 Terry Plank	1994-1999 Yanhui Wang
1953-1957 Wallace Broecker	1966-1971 John Webmiller	1976-1980 Donald Elthon	1987-1993 Cheryl Peach	1994-present Alexandra La Gatta
1954-1957 Paul Damon	1966-1972 Kenneth Wolgemuth	1976-1982 Michael Amdurer	1987-1995 Jennifer Reynolds	1994-present Colm Sweeney
1954-1959 James Cobb	1967-1972 Chi-Yu Shih	1977-1981 Dennis Adler	1987-1994 Miranda Fram	1994-present Elizabeth Gier
1954-1959 Leon Long	1967-1972 Mokhtar Hamza	1978-1983 Sara Langer	1988-1994 Franco Marcantonio	1994-present Guido Paparoni
1954-1960 Donald Miller	1967-1972 Tsung-Hung Peng	1978-1985 Steven Goldstein	1988-1994 Niraj Kumar	1995-present Arthur Greene
1956-1963 David Thurber	1968-1973 Shen-Su Sun	1979-1985 Andrew Herczeg	1988-1995 Steve Chillrud	1995-present Synte Peacock
1956-1963 Edwin Olson	1968-1974 Darlene Richardson	1979-1987 Marilyn ten Brink	1989-1994 Bernard Bourdon	1995-present Yongjun Su
1956-1967 Glen Erickson	1969-1974 Steven Emerson	1979-1986 Sherry Schiff	1989-1995 Jo Lin	1996-present Juliet Nichols
1957-1962 Edward Catanzaro	1969-1978 George Kippbut	1979-1987 Laurie Reisberg	1989-1995 Jordan Clark	1996-present Kathleen Donnelly
1957-1963 Marion Wampler	1970-1975 Douglas Hammond	1979-1989 Edward Garvey	1990-1993 Kevin Harrison	1996-present Marcia Tobin
1957-1964 Aaron Kaufman	1971-1976 Raymond Hesslein	1980-1986 Rik Wanninkhof	1990-1995 Roberto Gwiazda	1996-present Mark Potosnak
1957-1964 Fraser Fanale	1971-1977 Michael Perfit	1980-1989 Maureen Conte	1990-1996 Jerry McManus	1996-present Victor Engel
1959-1967 Ian Swainbank	1971-1977 Paul Quay	1981-1988 Richard Cember	1990-1996 Rachel Oxburgh	1997-present Malka Machlus
1961-1966 Fred McDowell	1971-1977 Thomas Torgersen	1981-1988 Susan Trumbore	1990-1998 Brenda Ekwurzel	1998-1999 Rose Came
1961-1966 Teh-Lung Ku	1972-1979 Curtis Olsen	1982-1988 Katherine Ellins	1991-1997 Keith Putirka	1998-present Elizabeth Cottrell



*"Ewing believed that if Lamont gathered  
as many cores as possible from as  
many places as possible, a pattern would emerge  
and mysteries would be solved.  
Thus, the dictum came down: a core a day."*





# A CORE A DAY KEEPS 'DOC' HAPPY

## *Building Lamont's Deep-Sea Core Repository*

by Rusty Lotti Bond

Like all libraries, it's quiet, somewhat cavernous and filled with items brimming with information and waiting to be tapped. The "pages" in this library, however, are made of mud.

Lamont-Doherty's Deep-Sea Sample Repository contains thousands of long, thin plugs of mud extracted from the bottoms of every ocean, including the ice-covered Arctic Ocean, as well as from lakes, rivers, corals, even a cow pasture in Chile. They are crammed to the ceiling in neat rows on shelves made of wire racks. Scientists from all over the world come to this library, just as they do to most libraries, in the pursuit of knowledge. In this case, it is knowledge about the Earth. They may borrow samples of the cores and in them they can read stories of climate changes, ocean currents, marine life, volcanic eruptions, deep-sea oil reservoirs—even sunken submarines.

Almost any part of the ocean a researcher wants to study has been cored by Lamont. When I wrote this, the core lab held 18,677 cores from 11,356 sites dotting the entire globe, with more still arriving every year from seagoing expeditions. It's a comprehensive library, and a rare one. Another one like it will not likely ever be created again. And it all began more or less by accident.

In July of 1947, Maurice Ewing—newly hired by the great but then-shipless university Columbia—was given the chance to use Woods Hole Oceanographic Institution's R/V *Atlantis* for two long, precious months. Seizing the opportunity, he stocked the ship with every instrument he could get or make

to explore every facet of the oceans that he could. Although he was a geophysicist, not an oceanographer or marine biologist, he also took along an instrument called the Stetson corer, invented by a WHOI scientist named Henry Stetson.

"I felt an obligation, with an expedition entrusted to me for two months, to get data of every conceivable kind," said Ewing, according to William Wertenbaker, in his book *The Floor of the Sea*. To man the device, Ewing also took along Stetson's assistant, David Ericson.

The *Atlantis* headed south, with scientists and crew taking measurements "of every conceivable kind" all along the way until they approached Bermuda. There, the echo sounder aboard ship told them that water depths were gradually decreasing. Hours later, the seafloor was one mile, rather than three miles down, because they found themselves over the flat top of a two-mile high seamount.

Here, Ewing decided, was a good place to try to get his first core. It took hours for the coring rig to descend to the seafloor and back, and the shorter the distance, the less he would pay in valuable ship time.

In a report dated July 25, 1947, called "M. Ewing's Account of MAR Station 1" (the 1947 *Atlantis* cruise was known as MAR1, for Mid-Atlantic Ridge), Ewing wrote: "About noon we started up a slope which turned out to be a flat-topped sea mount at about 1450 meters (790 fath.). I decided to work it."

Ewing later told Wertenbaker of the event, "Well, some people, you know, never get over their first anything,



their first drink, their first piece of tail. As it happened that core was one of the best of my life."

That first core and all the others taken on the 1947 cruise changed the concept of the seafloor forever. The prevailing view at the time was that the seafloor was a stable basket that caught the steady rain of particles that sank from the ocean surface—mostly the remnants of dead microscopic marine plants and animals: phytoplankton and zooplankton. Thus, the constantly accumulating column of sediment represented the whole of geologic time.

"In the old days," Ericson told Wertenbaker, "the theory was you could take a core anywhere at sea and get a complete history of the world from its creation—if you could take one that was long enough."

In 1947 few cores had been taken at all, and none long enough to disprove that theory. But in that first core from the Bermuda seamount, Ewing and company could see immediately that the ocean was far more complicated than they had imagined and that particles did not fall gently and uniformly to the seafloor. Instead, it was apparent that great unknown processes were at work, transporting sediments and otherwise disrupting the way they were deposited on the ocean bottom.

In his account, Ewing wrote: "The material on the surface of this important structure . . . showed it to be different from that presumed to lie on the adjacent deep sea floor. . . . What is the meaning of this difference in composition of bottom on the sea mount and on the surrounding deep ocean? Is it really a fact? This must be tested by a set of cores carrying from the deep ocean floor to the top of the sea mount."

The seeds were planted that many cores from many places had to be obtained to comprehend the ocean. But because it took so much time and effort (and money) to bring up cores at the time, and because scientists had made the assumption that the ocean floor was uniform, the few scientists who investigated cores routinely would spend years, even their careers, studying the few precious cores they could get. When the *Atlantis* cruise ended, true to his

promise, Ewing offered the cores to Stetson, but he turned down the offer, saying that he already had too many cores to study.

"So I thought I'd better study them myself or eat them," Ewing told Wertenbaker. Ewing had already figured that what he had found in the cores was going to upset existing preconceptions about the ocean basins.

That belief was strengthened when Ericson took the original cores back to Woods Hole, examined preserved plankton shells in them, and unexpectedly found that recent sediments lay directly upon forty-million-year-old sediments from the Eocene Period. Somehow, forty million years of intervening sediments were missing. Where had they gone? Furthermore, the recent sediments were coarse while the Eocene sediments were extremely fine-grained. Why were they different? At first, Ericson speculated that the deeper, older, fine-grained deposits could have been left after currents circulating around the seamount winnowed away coarser material. But the Eocene sediments showed no signs of such winnowing.

More cores were needed to answer questions like that, but to get them, Ewing realized that he had to find a way to make coring more efficient and economical.

The original ocean sediment corer, created by Charles S. Piggot of the Carnegie Institution before World War II, was an expensive, one-of-a-kind device. When the coring tube neared the bottom an explosive charge was set off, driving the corer into the mud. But friction built up quickly, stopping the corer from penetrating more than ten feet.

The Stetson model was far more simple: a long tube surmounted by a ton or so of lead. Ewing called it a cookie cutter. It took out a mud cookie one and a half inches in diameter and eight or ten feet long.

To get longer cores, a Swedish oceanographer named Börje Kullenberg put a piston inside the corer that was connected to the ship. When the coring tube was released, it slid past the piston inside. When friction between corer and sediments built up, a separation would be created

between the piston and the sediments inside the coring tube. That created a vacuum above the core. To remove the vacuum, the high pressure at seafloor depths would push sediments into the tube. In such a way, twenty-foot cores could be obtained.

But the Kullenberg device, though ingenious, was cumbersome and complicated. It took eight to ten hours to set up and get it to the bottom and another eight to ten hours to bring it back up and take it apart—nearly a full day's operation for one core.

Though Stetson's and Kullenberg's devices made strides, they both were still difficult and time-consuming to operate. Further, the coring tubes of both these devices were turned on lathes like gun barrels and were so expensive to make that their users were reluctant to deploy them, especially in risky places.

Those coring devices were fine if all you wanted were ten cores in your lifetime, but Ewing was aiming for thirty or more a month, and he presumed that difficult seafloor locations might also prove most interesting. To accommodate this exuberant coring style, Ewing made his own device—the Ewing Piston Corer—a design still used today that combined the best features of then-existing devices. Ewing's rig took plugs of mud averaging twenty to thirty feet in length.

He also made his corer out of two-and-a-half-inch steel boiler tube, an inexpensive commercial pipe, so that he could stock his ship with spare parts to repair or replace lost coring rigs. He was proud of his resourcefulness and the success of his simple but elegant rig. "This steel pipe, 2½ inches in diameter and twenty feet long, brings up samples of the ocean floor just as a housewife cores an apple," he said.

Ewing also turned to his ever-resourceful right-hand men, Joe Worzel, Lamont's assistant director, and Angelo Ludas, head of Lamont's machine shop, to work out other engineering problems. The two designed a new clamp that they nicknamed "the come-along," which allowed the coring rig to be lowered from ship to seafloor on just one continuous cable. This saved considerable time and effort.

They also solved another vexing mechanical dilemma: It takes far greater effort to drag heavy loads through water than it does through air, and the winches used at the time did not easily accommodate the great strain of lowering and raising the heavy coring rigs through the watery depths. Charles Piggot, who created the original corer, also had a winch for it. On what turned out to be Piggot's last voyage to collect cores, the flanges on the winch proved too weak and jammed the winch's rotating drum so that it would not turn. Piggot cut the cable, came home and never took a core again.

The Carnegie Institution, now out of the coring business but still owner of Piggot's winch, called the up-and-coming Ewing to ask if he would like the winch. Carnegie apparently tired of having two men on the payroll to maintain a winch that wasn't being used. In an interview, Worzel said Ewing turned to him, and Worzel replied, "A winch in the hand is worth two in the bush, even if it doesn't run. We can find a way to make it run." In typical Lamont style—economical and enterprising—the trio of Ewing, Worzel and Ludas hired a truck to haul the twelve-ton winch up to Lamont. In the Lamont machine shop they straightened out the bent flanges, redesigned supporting gussets to be strong enough to do coring work, and then took turns welding the newly designed gussets themselves.

"We all got pretty good at welding by the time we got done putting those gussets on the winch," Worzel said.

Soon they exchanged the winch's old diesel transmission for a torque converter, a device that was just starting to appear in cars. The converter provided enough strength to overcome the suction when it pulled the core out of the seafloor and, at the same time, enough control to prevent the core from erupting wildly (or "gallywomping" out, as Worzel put it). To pay out the cable lowering the corer more efficiently, they adapted and installed a Parkersburg Brake, manufactured by a nearby Pennsylvania company to allow big trucks to come down mountains without burning out their brakes.



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With such modifications, Lamont shortened the coring process from eight or ten hours to three hours. When Lamont acquired the *Vema* in 1953, it was ready to go coring in its own inimical and unprecedented style. That style was to collect as much data as possible, accumulating them to piece together a big picture that could be proved irrefutably. So it was with the cores. Ewing believed that if Lamont gathered as many cores as possible from as many places as possible, a pattern would emerge and mysteries would be solved.

Thus, the dictum came down: a core a day. Wherever the *Vema* went, and later wherever Lamont's second ship, the *Robert D. Conrad*, went, crews each day took at least one core, but often a few more. They were often guided to potentially interesting spots to core by Lamont's precision depth recorders, which precisely displayed seafloor topography. Ewing's credo was that the ship might never again get back to that's day location in the vast ocean, so grab a core while you had the chance.

He had neither the means nor the intention to work up all the cores the instant they arrived back at Lamont. And he ignored scientists who complained that he was wasting time and money by taking too many cores. He replied that he would stop when he found two cores that were alike. He instinctively knew that the mud brought up from beneath the ocean floor held stories—more stories than he, or all the oceanographers in the world working together, could possibly unfold. He decided to establish a library of cores so that researchers, now or in the future, could have specimens to study—with a fresh perspective or a more sophisticated instrument, perhaps—to discover something new.

Seeing the vast potential, both in the cores and in Ewing's zeal, Ericson left Woods Hole for the newly established Lamont Geological Observatory, becoming its first core specialist. When he first arrived at Lamont, he laid out his cores on a table under chandeliers in the Lamont family's former dining room. But the room was quickly overwhelmed by the core-a-day pace.

Jack Nafe, a Columbia faculty member and Lamont scientist, lived with his family in a cottage on the Lamont grounds and used an eight-car garage. It soon began to house the overflow of cores from the dining room and Nafe never got his garage back. The cottage he lived in is now called "Nafe House," but the garage became Lamont's first core repository.

The cores kept coming in. In the 1960s and 1970s, Lamont had two ships at sea, circling the globe and taking a core a day. The ship's captains competed to see who could take the most. One year the *Vema* was out for 288 days and took 381 cores; the *Conrad* was out 300 days and collected 250 cores. The *Vema* may have won that round, but later the *Conrad* brought in an incredible 437 cores on just one cruise. One year, 631 cores were returned to Lamont. Lamont ships continually set records for taking the most geographically northern and southern cores, cores from the deepest water, and the longest cores. One core taken in the central Pacific was an incredible ninety-two and a half feet long!

The ships always had room for more cores, no matter how cramped accommodations were on board. *Vema's* hold was crammed with perilous-looking wooden racks to store the long tubes of sediment while in transit. Everyone accepted that science always came first and comfort second.

Doc's idea of a core "library" was very quickly becoming a reality. Soon a second story was built over Nafe's one-time garage to house a laboratory dedicated to work on the deep-sea sediments, the first Core Laboratory. An open slit was constructed in the floor of the upper level so that long sections of core could be lifted up from the ground floor into the lab area.

Almost immediately, the cores proved their scientific worth. Among the first mysteries they helped solve was the one in that first core off Bermuda—why recent sediment lay directly on ancient sediment.

A series of cores was collected from the walls and the valley of the Hudson Canyon—a vast submarine canyon as remarkable as the Grand Canyon on land—

which emerges from the mouth of the Hudson River, bites deeply into the continental shelf and extends all the way to the flat abyssal plains of the ocean hundreds of miles off the coast. Other cores were taken on the Grand Banks off Newfoundland, where several undersea transatlantic telegraph cables were mysteriously severed during a 1929 earthquake. Another core from the deep Puerto Rican Trench had the shells of a microscopic plant that lives in coastal waters.

The pattern that emerged from these series of cores all pointed to a phenomenon called turbidity currents. These are torrential underwater currents of water and sediments, set into roiling motion by a landslide on the slope or by an undersea earthquake. They thunder through the oceans with enormous speed and force and carry huge loads of material great distances until the currents lose momentum and the material at their front edges settles meekly into a flat plain. Turbidity currents in the ocean explained how canyons were cut, cables were broken, sediments were transported, abyssal plains were formed, and how offshore oil deposits (ultimately formed when organic matter is buried by sediments) were created.

The discovery of turbidity currents also resuscitated dashed hopes that researchers could get a clean record of Earth's past history from the cores: Once the disruptions in the sediment sequences could be explained, they could be factored out. The cores quickly became cornerstones for research on climate change.

Locked in the cores was a wealth of climatic information. The remains of various microscopic shells in core layers revealed telltale evidence of past oceanic conditions: Different kinds of marine life in the sediments signaled changes in ocean currents, which brought different species to different regions at various times. The presence of marine life that thrived in either warm or cold waters revealed changes in surface water temperatures. From the ratio of two types of oxygen isotopes in preserved shells, scientists could even tease out the amount of Earth's water that was locked up in ice sheets on land, and by extension

and with corroborating evidence, they could deduce past global sea levels and ice ages.

The field of paleoclimatology (the study of past climates) blossomed even more quickly because of the synergy created by the proximity of Lamont's budding geochemistry laboratory. In the early 1950s, Wallace Broecker and J. Laurence Kulp at Lamont developed radiocarbon dating techniques to date fossilized shells in Ericson's deep-sea cores. So in the mid-1950s when Ericson and his associate Goesta Wollin identified an abrupt boundary in a core—marked by an abundance of shells from a species that thrived in warm water above it and none below it—they could send samples to Broecker and Kulp, who dated the boundary at about 11,000 years ago. In retrospect, that boundary roughly marked the end of the Earth's last glacial period and the beginning of the current relatively warm epoch known as the Holocene Period.

Suddenly scientists could attach dates to various layers in the cores and establish a top-to-bottom chronology of events. They were further aided by evidence in the cores of reversals in Earth's magnetic field, whose timing was also quickly becoming established at Lamont. Lamont scientists Neil Opdyke and James Hays, along with graduate students Billy Glass and John Foster, embarked on a mission to identify a record of magnetic reversals in the cores—providing a geologic calendar on which climatic events could be superimposed.

But evidence from one core in one location, or even a few from several, could not make the case for how or why the entire Earth's climate changed. When the scientific community sought to answer the age-old question of what caused Earth's cycle of ice ages, they needed to correlate many cores from all over the Earth. Naturally, Lamont's Core Lab provided the unique resource.

The task was too daunting for one scientist. It required a wide range of expertise: geochemists, mineralogists, oceanographers, paleomagneticists and geophysicists. It was even too big for one institution. So Jim Hays and John Imbrie, a former Columbia and Lamont scientist who had just moved to Brown University, concocted a plan quite



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along the lines of another Lamont tradition—organizing and guiding multidisciplinary, multi-institutional research efforts to tackle ambitious global scientific problems. Launched in 1971, it was called CLIMAP (for “Climate/Long-Range Investigation Mapping and Predictions”), and its mission was to reconstruct the history of ocean changes deep into the past.

A central CLIMAP office, was established at Lamont to coordinate the research activities of nearly 100 scientists from the United States and seven other countries. Analyzing the climatic signals from numerous cores, the CLIMAP team proved that changes in astronomical cycles (more specifically, changes in the Earth’s eccentricity, precession and tilt) triggered Earth’s glacial cycles. In 1976, Hays, Imbrie and Nicholas Shackleton of the University of Cambridge published their classic paper in the journal *Science*: “Variations in Earth’s Orbit: Pacemaker of the Ice Ages.”

What helped make, and continues to make, Lamont’s Core Laboratory so valuable was the remarkable foresight of Ewing, Ericson and Wollin in the early days to record so meticulously essential information about each core. They developed a system for archiving the cores that continues today at Lamont and became the standard for subsequent collections.

Then, as now, a similar procedure is followed when a core arrives at the lab: First, it is split open longitudinally, and each half is placed into a tray. Intervals are marked with plastic tabs every ten centimeters down the length of the core. In earlier days, each core was photographed at this point, the images carefully mounted and the film negative filed as a permanent record. Today the images are digital. Colors in most cores begin to change moments after they are opened and exposed to air, heat and light. The photographs preserve original color variations, and the interval markers make it possible to reconstruct the variations years after the core has dried to a homogeneous tone. Some cores have been sampled so heavily that the image is all that is left, with the documented original color variations themselves becoming the data!

The core halves are placed in five-foot-long tubes that are sealed on both ends and then refrigerated. Until 1985, half the core was encased in plastic, and both halves were stored in a non-refrigerated environment. In both cases, one half becomes the “archive” to be saved for future work; the other half is called the “working” tray from which samples are taken. (Today, all the archive halves are refrigerated.) A superficial description called a “megascopic description” is made, recording the visual color of layers, changes in type of sediment, and basic lithology (rock types) and micropaleontology (preserved shells).

In Ewing’s days, everyone worked on the cores at Lamont: splitting the cores, labeling, photographing and then mounting the photographs, filing the trays of split cores in well-ordered banks of racks. To process the tremendous number of cores coming in, each worker had to spend one day a week generating megascopic descriptions. Wollin looked after all the archiving with Ericson’s guidance. The first full-time curator, Chuck Frey, was appointed in 1957, the International Geophysical Year, which marked the beginning of a surge in funding support for large-scale oceangoing research. Over Lamont’s first half-century, four other curators have overseen the Core Repository: Roy Capo, David Turkel, Floyd McCoy and the present curator, me.

All this information for the past fifty years has been filed into ringed binders referred to as the “Core Logs.” The books also contain a “Ship Log” of information recorded at the time of the actual coring aboard ship: coordinates of the site, water depth, comments on retrieval time, weather conditions, and any information that might someday be useful to a researcher. (These originals are frequently bordered by non-essentials such as doodles, amateur illustrations of failed corings, rough comments on the ability or disability of a winch operator, mud streaks, grease, and sometimes just good humor.) There are now 850 volumes of core data in the Lamont repository.

From the beginning, and with few exceptions, all core sampling also has been documented: who, when, how much, and what for—all bound into another large set of volumes.

Fortunately for anyone looking for data on a particular core, nearly all the information in the Core Logs is now on a digital database, which includes the geographical coordinates of the holes where cores were taken, water depths, core lengths, and dates they were taken. Over time, the list of data entries has grown to include codes for different structures in the core, including bedding or grading, evidence of disturbance, burrowing, eighteen possible entries for paleontology (such as foraminifera, plant debris, or ostracods), twenty-eight entries for mineralogy, from ash layers to zeolites. Lithology includes ooze, silt, sand, and on and on.

The format of Lamont's huge database has served as a prototype for other databases of similar information. And we continue to update the Lamont database and incorporate additional types of information. One can now search for publications resulting from work on the cores. Written descriptions of the cores have been digitized. All of this is searchable on the database. One can retrieve a core containing "pteropods" from any ocean in the world, or *all* the cores in the world containing tektites. All the photos of the cores are in the process of being digitized and added to the database, so that Web surfers will be able to view a particular core from a particular part of the ocean from their own computer monitor.

People can still view cores, find data and read research results at the repository itself, so it functions in many ways as a museum, as well as a library. Because most of Lamont's coring cruises were funded by federal agencies, scientists may "borrow" materials—in this case, sediments. They usually can't return the sediments; instead they return data and knowledge.

In the early days, any scientist who could get to the cores could take samples. As the field of marine geology grew and more people became involved, more samples were being taken from the cores and a more formal approach had to be established.

Scientists are now required to submit a written request to take samples, which is reviewed by a sample committee. Early on, non-Lamont people were restricted from working on a Lamont core if someone from Lamont

was working on the core and the same topic. Once, someone who was denied sampling access to some cores came into the lab at midnight, took samples back to his own lab, and soon published his work on the sediments in a major scientific publication. Eventually, the non-Lamont restriction was eased, and now decisions are made more on the basis of benefit to the scientific community. The committee does try to ensure that cores are not used up by redundant projects or by unnecessary oversampling.

Still, occasional turf wars over the sediments do arise, when someone believes his or her work merits priority. There was a Lamont scientist who hid whole trays of cores so that other researchers could not find them. But he forgot where he hid them and his ignoble deed was discovered.

The usual amount of material taken for almost any work is a half to five cubic centimeters of material, up to about five grams. The most memorable deviation from those standards was a visiting investigator who confidently claimed he was quite familiar with sampling deep-sea cores. We encourage researchers to see and feel the cores themselves, and we honored his claim. Soon after he went to the core racks to begin sampling, a lab worker came to me stuttering and breathless, saying someone was sampling in a tray with a geologist's pick hammer, a tool more commonly used to whack off a piece of rock from the side of an outcrop. In his sample bags were six- to ten-inch long chunks of core that he had hacked out with his pick hammer.

Use of the core library has remained constant over the years, with scientists pursuing information in assorted fields. One of the more consistent geological phenomena studied is volcanism.

In the 1960s, cruising off the west coast of Mexico, Joe Worzel detected a reflection with ship's echo sounder from a curious sediment layer. He tracked this layer over several days—over some 300 miles off the coast and another 1500 miles down the western South American coast. When the sediment boundary appeared to rise close enough to the surface, Worzel took a core. (The sediments were green and had a hydrogen sulfide smell. Worzel jokingly



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sent a message to Ewing that he had found remnants of lunar green cheese in the Pacific, later reporting that Ewing did not find this humorous.) But more important than the green layer, the core also contained a relatively thick layer of what he called white ash. Back at Lamont, Ericson identified the material as ash from a volcanic eruption—one that must have been almost unimaginably enormous to cover the seafloor so extensively.

In 1965, a core taken from the Mediterranean, RC9-181 (the 181st core taken on the *Robert D. Conrad's* ninth cruise), revealed a nearly continuous record of several volcanic eruptions spanning 450,000 years, including a recent one, only about fifteen centimeters down from the top. That layer came from the 79 A.D. eruption of Mount Vesuvius, which devastated Pompeii. This was the first deep-sea core that correlated an archeological and a geological event, an important mechanism for precise dating. Each eruption from a volcano emits particles with unique chemical signatures that scientists can use to identify specific volcanoes and eruptions.

Sometimes there are unexpected uses for cores. When the U.S.S. *Thresher*, a Navy submarine, was lost at sea in 1963, descriptions of cores in the Lamont collection aided the Navy's fact-finding commission on the loss. In light of Lamont's early work on turbidity currents, AT&T Bell Labs studied Lamont core material to characterize the natural seabed radiation along proposed corridors before laying fiber-optic cables in both the Atlantic and Pacific Oceans.

Cores from Long Island Sound are being studied to measure pollution from a sludge dump site in the New York Bight, and from other coastal areas of the continental United States. Cores from the Hudson River have been used to study contamination from toxic industrial polychlorinated biphenyls (PCBs). Cores from Mono Lake in California have been used to study the cycles by which carbon travels through the Earth system. That aforementioned core from the Chilean cow pasture was used to study ice ages and other climate cycles, as were cores retrieved from platforms built on floating sea ice in the Arctic Ocean

called Drift Station Alpha, Drift Station Charlie and Ice Island. More recently, cores from corals have been used to glean precise records of ocean temperatures and other conditions to reconstruct the past history of sea levels and of El Niño cycles.

Fifty years after those first cores were taken on the *Atlantis*, and stored and worked on in the elegant Lamont estate dining room, researchers still request mud from the earliest cores of the Lamont collection, and they find new information in old cores.

In 1988, in cores from another collection, a German oceanographer, Hartmut Heinrich, found six layers of tiny white carbonate-rich pebbles in sediment cores from the North Atlantic. The pebbles, he said, had been scraped off continents by advancing glaciers, frozen into their bases, and carried out to sea when the glaciers "calved" into icebergs at the coastline. Once afloat, the ice eventually melted, dropping telltale pebbly debris onto the seafloor. The layers of so-called ice-rafted detritus (IRD) came at regular intervals, signaling a previously unknown, regularly paced climate cycle that periodically launched armadas of massive icebergs every 7,000 to 10,000 years. These cycles occurred within the last ice age.

The discovery of so-called Heinrich Events (along with corroborating evidence of past air temperatures from a recently cored ice sheet atop Greenland) helped launch a new front in climate research. Scientists suddenly began wrestling with the idea that Earth's climate system can shift rapidly and abruptly—with effects perhaps not so dramatic as the waxing and waning of ice ages, but still substantial enough to disrupt life on the planet. Could it shift again in the future—especially if stressed by the greenhouse effect?

The discovery also sparked a dramatic research shift for my husband, Gerard Bond, a Lamont geologist who had previously specialized in studying rocks on land. Suddenly, those rocks were showing up on the seafloor and could be analyzed for clues to past climate change.

He went down the hall, pulled out cores taken twenty-five years before by the *Vema* from opposite sides of

the North Atlantic, one from off the southwest coast of Greenland and the other more than 600 miles to the south, off the coast of England. Analyzing the cores even more meticulously, he and colleagues found regularly spaced layers of microscopic rock particles that originated from Greenland and Svalbard, an island in the Arctic Ocean, as well as glass from Icelandic volcanoes. The tiny particles had been transported by glacial icebergs and sea ice to the North Atlantic, deposited on the seafloor and buried by subsequent sediments.

The regularly spaced layers indicated that on average, every 1,470 years, plus or minus 500 years, cold, ice-bearing waters, which today circulate around southern Greenland, pushed as far south as Great Britain. More evidence came from analyses of plankton remnants in the sediments, which showed that cold-water-loving species thrived whenever and wherever IRD increased.

The combined evidence pointed to a newfound, naturally occurring, 1,500-year climate cycle, in which average temperatures throughout the North Atlantic region dropped within a century or two, stayed cold for several hundred years, then warmed again as quickly as they had cooled.

Evidence in the cores showed that this 1,500-year cycle has continued uninterrupted over at least the past 80,000 years. In his most recent work, Bond has shown that the Little Ice Age, a historically documented cold spell that gripped the world in the seventeenth and eighteenth centuries, was the most recent manifestation of the phenomenon.

These periodic sudden cold spells occurred when the Earth was covered with massive glaciers during the last ice age and persisted even after human civilization began to flourish in a relatively warm, ice-free Holocene Era, which scientists had previously thought was resistant to dramatic climate shifts. The newly discovered cycle appears to be a pervasive component of the Earth's climate system—a pacemaker of rapid climate change. Finding the mechanisms that govern this cycle will provide a fundamentally new understanding of how Earth's climate system works. That understanding is essential for guiding people who are currently

trying to figure out how our climate system will respond to potential global warming induced by the rapid buildup of industrial greenhouse gases.

The increase in rocky particles in the sediments indicated an increase in floating ice. Did cooler air temperatures cause glaciers to advance and sea ice to spread? Did polar waters penetrate a warm North Atlantic current that prevails today, and disrupt the global ocean circulation pattern that keeps the North Atlantic region warm today? Or did cooler ocean temperatures allow more ice to survive long transits before they melted? The melting ice, in turn, may have added fresh water to the North Atlantic and disrupted the delicately balanced global ocean circulation system, known as the Great Ocean Conveyor, which is set in motion by the sinking of denser, salty water in the North Atlantic. Any disruption of the Conveyor may well have had far-flung, worldwide effects.

All these answers and all these questions lay hidden for decades in the cores, waiting for someone to reveal them, or to look at them again with new eyes or in a new light. Sitting on shelves in Lamont's Core Laboratory are thousands of cores, many untouched, from waters where no one else has passed. What secrets do they hold and where will they take us in the future?



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*"You don't collect much data when your ship is  
in port, tied to the dock."*





# SHIPS AND SUCH

## . . . Gathering the Flowers of the Sea†

by Dennis E. Hayes

I was standing in Doc Ewing's office, a twenty-two year-old graduate student, when the late-night phone call came in: John Hennion, chief scientist aboard the *R/V Vema*, had died in an explosives accident aboard ship.

Doc was calm on the outside but he was badly shaken, even to my uninformed eye. He went about relaying the tragic news to John Ewing, his brother and scientific colleague, to Joe Worzel, Lamont's associate director and Doc's steadfast right-hand man, and to Arnold Finck, Lamont's longtime administrator. They formed a cadre of supporters to travel with him that long two miles to Piermont to inform Nan Hennion that she was now a widow. I was swept along with the group, wondering with my two-month perspective as a student, "Does this kind of thing happen often?"

Seismic operations were immediately suspended on *Vema*, in part because most of the explosives aboard ship had been frantically jettisoned while the ship was ablaze. But more important, no one knew how or why this deadly accident had occurred.

Doc's public mourning was brief. He was soon on a plane to Tierra del Fuego, South America, to take direct charge of the science operations and to resume Lamont's relentless global exploration of the seafloor using seismic waves—an exercise that required the shooting of explosive charges every minute around the clock.

Five months later, I was aboard the *Vema*, working in the Labrador Sea and nervously taking my turn shooting those same explosives that Hennion had, along with the rest of the

scientific crew. I tried not to think too hard about "it," but found myself concentrating intensely when I was in the hot seat. The whole activity defined an unspoken "the-show-must-go-on" mind-set of Lamonters: Overcome obstacles, seize opportunities, don't waste either time or money, keep sailing. Whatever it took, Lamonters were driven to collect the data that drove the science.

I soon learned that Lamont's *modus operandi* was to keep its vessels at sea, each continuously collecting data for about 330 days per year. When a colleague from the Woods Hole Oceanographic Institution once chided Ewing that Lamont did not have a "proper" home-port facility, Ewing remarked matter-of-factly, "You don't collect much data when your ship is in port, tied to the dock." This unambiguous work ethic set the tone for everyone at the observatory from Day One.

I arrived in New York in early February 1961 amid a colossal snowstorm that shut down the city to all but emergency traffic for three days. I had traveled thirty hours via bus from snowy Kansas and quite unknowingly avoided being stranded at the Port Authority by a matter of minutes. I caught the last bus that would head to New Jersey for several days. Somehow I found my way to suburban Oradell, New Jersey, where I stayed temporarily with my cousin.

That year, Charles Hollister and I were the only two graduate students entering Columbia's Geology Department in midyear. He was from Oregon and rural California and the two of us were so green that Charlie (I swear it was he) walked up to a New York City subway token booth clerk and asked for a roundtrip ticket to Columbia.



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At that time Columbia's Geology Department and the Lamont Geological Observatory were anything but a tightly integrated scientific unit. The classic geology disciples were well-ensconced in Schermerhorn Hall on the Morningside campus, while the avant-garde Lamont geophysicists, geochemists and oceanographers were aggressively building their research programs at Lamont under Doc Ewing's strong direction.

This caused some tension for students, especially those with a foot in both camps. Most of the tension, however, existed between Columbia's central administration and the steadily growing autonomous research enterprise housed on the Lamont estate, which seemed to make Morningside nervous. Early on, Ewing once told me, "If you ever need help or anything on the Morningside campus, *don't* mention my name."

When I arrived at Lamont, I first looked for a place to sit. Most everyone was situated in Lamont Hall, formerly the main residence of the Lamont family. A prefab Butler-type building housed an electronics shop and the observatory's administrative and marine affairs staff. The campus also had the first segment of the geochemistry building (it was ugly even then), where the geochemists dwelled. We geophysicists thought they were an odd lot and joked that they tried to identify chemical properties to five significant figures and apparently inhaled a lot of strong substances for a good part of each day.

The geophysicists resided in Lamont Hall, and young ones who were already distinguishing themselves, such as Chuck Drake, Bruce Heezen, Manik Talwani and John Ewing, were rewarded with their own offices—though typically these were converted closets, bathrooms or porches. Although I was designated to be Ewing's research assistant, I soon discovered that students were lucky to have their own chair, let alone an office. Space and amenities were at a premium. In the dark, damp basement of Lamont Hall, a small electronics shop had pipes so low that headroom was limited to five feet in places. Bruises on our foreheads testified to excursions into this dungeon.

But by summer I was out on the open ocean. My courses completed for the semester, it was time to get a different type of schooling. You didn't need much experience to go on

a research cruise in those days. In fact, that's where you got your experience. It was part boot camp and part on-the-job training. Besides, with all that data out there waiting to be collected, Ewing needed all hands on deck. Graduate students provided particularly cheap labor. Most of us went to sea once or twice a year. As the lowliest of student technicians, in 1961 I boarded Lamont's first research vessel, the *Vema*.

She was originally christened *Hussar*—a 202-foot, three-masted schooner with teak decks and a wrought-iron hull built in 1923 in Copenhagen for the investment banker E. F. Hutton. Below deck, she was luxuriously appointed, with a Louis XV bedroom, an Edwardian sitting room with a marble-rimmed fireplace and oriental rugs, a dining salon with stained-glass windows, bathrooms with gold faucets. In 1934 she was bought by Georg Ungar Vetlesen, a shipping magnate, who renamed her *Vema*, after the first two letters of his family name and his wife's name, Maud. Like all oceangoing yachts in this country, she passed to government ownership during World War II, at first patrolling coastal waters for the Coast Guard. Later she underwent a drastic conversion to a floating barracks and training ship for the U.S. Merchant Marine, losing her gold faucets and other amenities. After the war, she lay abandoned and aground on mud behind Staten Island for several years, until she was salvaged by a Nova Scotian captain for use as a charter vessel. How Lamont got her is an interesting story, first told to me by Joe Worzel:

In the early 1950s, Worzel and Chuck Drake had modified a Navy tug called the *Allegheny*, which was supposed to be used jointly by Lamont and Hudson Laboratories, another Columbia research lab across the Hudson River. When Lamont's turn came in 1953, the Navy said Hudson Labs needed it for urgent classified work. Ewing protested to the Office of Naval Research (ONR) that Lamont had hired personnel and prepared equipment in anticipation of a cruise and demanded that ONR charter a ship for Lamont. The Navy agreed, and in typical fashion, Doc instructed Worzel to find Lamont a ship.

Worzel scanned ads in yachting magazines and saw one for the *Vema*. He contacted Captain Lewis Kennedy, went to Nova Scotia to see the ship and determined that she

had plenty of room to accommodate Lamont's research operations. The engines weren't working but he was assured that they would be soon. Lamont chartered the ship, and Worzel suggested adding a clause with an option to buy it, in case "we liked it well enough," he said. The \$20,000 charter fee would be applied toward a purchase price of \$100,000.

*Vema's* first cruise for Lamont, *Vema 1*, went to the Caribbean and the Gulf of Mexico and the scientists "liked her well enough," so Ewing sought funds to purchase the ship. On the last day of the charter, which expired at midnight, he called Worzel at noon at the shipyard, where the ship was being loaded with equipment for Lamont's next cruise, and told him he had been unable to raise the money to buy the vessel.

Worzel told Ewing that the observatory would never get a such a good shot at getting a good ship so cheaply. Ewing put in a call to Joseph Campbell, Columbia's treasurer at the time, only to learn he was gone for the afternoon. Undeterred, Ewing called Campbell's home. His wife said her husband was playing golf. But she was persuaded to drive to the golf course and find him. An hour later, Campbell called Ewing.

Ewing convinced Campbell that the budding observatory had to have the ship. He guaranteed that it would make Columbia proud and he also guaranteed that somehow he would raise \$80,000 to pay Columbia back. It was now much too late in the day to secure Columbia funds. Campbell agreed to put up his own money to buy the *Vema* before the midnight deadline, but he instructed Ewing to let him break the news to the Columbia trustees.

Shortly after 5 p.m., still in his golf clothes, Campbell bought the *Vema*—much to the regret of Captain Kennedy, who by now had realized the ship was worth a lot more than \$100,000 and was reluctant to sell. He had no choice, though, but to go ahead with the deal. Campbell secured insurance on the ship immediately so it would be covered after transfer of ownership. Ewing no more than returned to Lamont when he got a phone call from an irate Columbia trustee—the one whose insurance company had been contacted to insure the ship!

Ewing continued to have difficulty raising funds to pay for the ship, so Worzel went down to Washington and

told ONR officials that Columbia would insist on selling the *Vema* if money couldn't be raised. He persuaded ONR to amortize the purchase cost of the *Vema*, as part of its funding for Lamont's ship operations, over eight years, \$10,000 per year. In fact, ONR allowed Lamont to charge for ten years, so that actually Columbia "made money" on the deal. Lamont didn't fool the Navy. ONR was quite aware of the finances: The Navy either allowed amortization on the entire \$100,000 purchase price, or it provided for interest charges.

Columbia garnered a great deal of pride and the Navy got more than its money's worth, because no research vessel collected more data from more parts of the unexplored ocean as efficiently as *Vema*.

In those days, ships of other great oceanographic institutions tended not to venture too far from their own territorial waters, with Woods Hole, for example, working in the North Atlantic and Scripps Institution of Oceanography ships working the Pacific east of Hawaii. But from the start, the *Vema* embarked on a near-boundless mission to explore every ocean. In 1959-60, *Vema* became the first research vessel to circumnavigate the globe and cross both the Antarctic and the Arctic circles on a single cruise and in a single year.

Ewing expected scientists to use the ship every minute of every day. While scientists at other institutions typically collected data solely for their own individual research, Lamont scientists had standing orders directly from Ewing to collect as much data and as many different kinds of data as possible—regardless of whether the scientists aboard had any personal interest in them.

Lamont was the first institution to routinely collect precision depth recordings of the seafloor, seismic reflections of the layers below the seafloor, gravity and magnetic measurements, probes of the heat flow through seafloor, as well as seafloor sediment cores and ocean bottom photographs. Quite unlike anyone else, we were collecting all that data all the time. Everyone took turns standing watch, so that data were logged twenty-four hours per day. Everyone on the ship was responsible for a particular area of data acquisition, so that if the gravimeter wasn't working at two a.m., for example,



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there was someone aboard whose rear was pulled out of bed to fix it fast.

From the time we left port and got out of traffic, the watches started, the equipment went into the water and was turned on and kept running until we got to another port. It was unusual on a thirty-day port-to-port cruise to have as much as six hours without data.

Once or twice every day, we stopped to sample a part of the ocean floor. The prevailing attitude, simply put, was: "You're paying good money to have a ship here and you may not ever get here again, so make the most of it. Collect all the data you can and do it simultaneously."

Lamont was the first to operate two wires over the side of a ship at one time. On the wires, we sent piston corers, heat flow devices, cameras, water samplers and other instruments to the ocean bottom. It was a difficult and potentially dangerous operation. The lines were separated at the surface on the ship by not more than thirty to forty feet in 3,000 to 4,000 fathoms (18,000 to 24,000 feet) of water, and it took great care to keep the wires from getting tangled. But we had it down and could take a two-wire station—and stop the ship from everything over the side, to everything back and ready to go—in two hours.

Joe Worzel had developed and had carefully written out the protocols for the breakthrough two-wire operation, and if you came back from a station with just a core, he'd ask, "What the hell were you doing with the other wire? Was the winch broken or something?"

Every time we put an instrument in the water, we risked losing it, but we were careful and we rarely did. When the inevitable did happen, we wouldn't let it slow us down. Once our winch failed on the first day of a cruise and we had to make the painful decision to cut the line and lose our heat flow probe, coring apparatus and 12,000 feet of cable. So we made another heat flow device out of an old airgun chamber and parts cannibalized from other equipment aboard ship. We were used to building instruments. Until the early '70s, little if any oceanographic instrumentation could be purchased from manufacturers, so students, scientists,

technicians and machinists alike worked side by side to design and fabricate some new or better widget. Our jury-rigged heat flow device was a monster that weighed 200 pounds, rather than the normal thirty pounds, and took three men to lift. It didn't work as well as the real device, but it was better than the unthinkable: no data.

We also built instruments that caused wholly new scientific fields to blossom. Before 1959 it was not possible to make gravity measurements at sea except by using an extremely cumbersome pendulum apparatus that took an hour to yield a single measurement. Even then, the apparatus could only be used in submarines, which rode in calmer waters well below surface waves, thereby minimizing the distorting roll and pitch and associated accelerations created by the vessel's motion.

Following in the footsteps of the noted Dutch geophysicist Vening Meinesz, Worzel and, to a lesser extent, Manik Talwani started collecting gravity measurements for Lamont. Worzel pioneered the use of gravimeters on surface vessels by placing them atop gyrostabilized platforms. These were hardly off-the-shelf items, and the early ones were crafted at Lamont by Worzel, Harry Van Santford in the machine shop, and others. One of my early assignments was to test "Mable"—the nickname for the "stable table" for the gravimeter. Tests were conducted on the beach to determine just how well Mable could maintain a horizontal position in the presence of simulated ship motions. We built a contraption that could mimic the pitch and roll of vessels and placed inside of it the stable platform and gravimeter. Using a scheme of light beams, mirrors and photocell sensors, we could detect whether the stable platform was experiencing any residual motion. I wrote my first published paper in 1965 on this topic.

Using the gyrostabilized platform approach, gravity has been measured continuously aboard Lamont ships since 1960, with ever-increasing accuracy. Gravity measurements reflect the density distributions of the seafloor. Thick, elevated areas such as seamounts are represented by greater mass and thus very slight increases in the strength of the local gravity field. In the 1960s and 1970s both Worzel and Talwani

played vital leadership roles in interpreting gravity measurements to decipher contrasts in the distribution of mass caused by the varying deep crustal structures beneath the seafloor.

Gravity was one of several examples of measurements that vindicated the Lamont philosophy that new instruments or methods led to more data, which led inevitably to more discoveries. Ewing was openly criticized by many colleagues for collecting data for which he had no particular use in mind, but he was sure the data contained some valuable information about the Earth that eventually would be useful. That certainly turned out to be true, for example, when Jim Heirtzler and Walter Pitman at Lamont used magnetic data to prove that the seafloor was spreading, that is, moving away from both sides of mid-ocean ridges. It was a critical discovery that helped clinch the validity of plate tectonics theory, which revolutionized our understanding of our planet. And scientists today continue to glean new information from Lamont's library of cores, many of which were raised from the seafloor before those same scientists were born.

Once after a thirty-five-day cruise, I remember pulling into Jamaica in the early afternoon—time enough to take a core in the harbor of Kingston Bay, Ewing insisted. Everyone moaned and groaned. We had been at sea for a long time on a tough leg and we could hear calypso music from the shore. We took that core and had trouble doing it. By the time we finished, all the customs officials had left and we had to spend the night on the ship, within swimming distance of the shore and all its bars and R&R activities.

Even when he wasn't aboard, Ewing had a strong hand in every aspect of ship operations. It was a colossal challenge to define and devise the most valuable research projects and to man the ships with technical support and chief scientists to implement them. In fact, he devised an elaborate numerical code designed to minimize the cost of telexing daily ship reports back to Lamont. It was speculated that Ewing was the only one at Lamont to monitor this information flow. It was certain that if you made any coding error while at sea, Ewing would brusquely bring it to your attention within a day.

When a cruise was completed, Ewing did not trust having any of the data shipped back. In those days, the data were stored on paper, not in computers, and if they were lost, they were *lost*. It was the chief scientists' responsibility—one they took very seriously—to carry data by hand, often in many suitcases or canvas bundles, back to Lamont.

At most labs at the time, the chief scientists individually controlled data collected at sea, and some ended up in bottom drawers, never to see the light of day. Not at Lamont: Data collected on Lamont ships were *institutional* data. Within a week after they arrived at Lamont, more typically within the first few days, all the seismic data collected on a cruise were laid out on the table and the Ewings, the chief scientists and whoever else was interested would examine them.

This was the culture that Lamont students learned. We marched under the watchful eyes of a benevolent dictator who said, "You must collect this information, whether you have any interest in it or not, process it carefully, take it back and make it part of a vast data library." Rallying around this common goal, working together under extreme circumstances, we were a close-knit group. So while a guy like Worzel could be irascible when there was a ship to be outfitted or a technical problem to be solved, his gruff exterior inevitably would fall away to reveal a kind and committed man who with his wife, Dottie, invited students into his home and treated them like part of his family.

The Nafes, Jack and Sally and their daughters Macky and Kate, were also an integral part of the "Lamont Family." I was often the beneficiary of their hospitality and persuaded myself they couldn't possibly be this cordial to all the students. Jack, a longtime professor of geophysics at Lamont, was equally generous as a teacher. He loved to teach, and students who didn't feel like being taught on a particular day learned to stay safely clear of his office. He had a unique approach to problem-solving that started with basic principles. If there were no slip-ups along the way, somehow, with student in tow, the answer to a complicated physics or math problem eventually emerged with Nafe confidently smiling and me or some other captive student standing stunned and impressed. He went over every word of my thesis with me—an experience that marked a



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turning point in my life by giving me confidence in my ability to express myself through the written word.

Ewing, too, by word and deed was a consummate mentor, though he was harder to get close to—mostly because he was always working full-tilt. He was rarely seen without a pencil and notebook in which he was writing all the time. At Lamont's weekly Friday colloquia he would, of course, come with his notebook and sit in the front row and begin writing from the instant the speaker would say, "I'm so-and-so from such-and-such institution." It unnerved more than a few lecturers.

Once when I was working with him in his office, I asked him what he used those notebooks for. Mostly nothing, he replied. So why on Earth do you have them? I asked. Well, in the case of the colloquia, he said, I write in them to keep myself awake.

Though he usually opted for work over socializing, Ewing didn't begrudge us students a good time and kept a beneficent eye on us. For years, students and scientists played touch football during lunch hours. Different research groups—seismology, oceanography, marine geophysics, geochemistry—each had a team. We played on the field between Lamont Hall and Ewing's house, and we were pretty noisy, but Ewing never complained. Sometimes he would walk past and seem happy that we were having a good time.

Once Lamont's head of buildings and grounds discovered several broken panes of glass in the sun porch of Lamont Hall. We football players were the most obvious suspects and the B&G director complained to Ewing. But we hadn't done it. The B&G director held fast that we were responsible and continued to complain to Ewing. One day Ewing and I were talking about something and he said, "Gee, I really would like to know: Did you guys have anything to do with breaking the windows?" I told him, "Absolutely not, and if we had done it, we would have told someone about it."

Ewing fired the B&G director. But not before he chewed him out, saying, "These are my scientists. These are the people I entrust the ship to."

Loyalty was sacrosanct to Ewing. For the most part, it created a sense of family. Lamont was actually renowned

for the *absence* of intra-institutional bickering, turf wars, and professional isolationism that were infamous at several competitor institutions. But no family is perfect. And while Ewing could inspire loyalty, he also demanded it.

That may have been at the root of the legendary Lamont schism between Ewing and Bruce Heezen. Ewing had recruited Heezen as an undergraduate and from the first days of Lamont, the two had worked closely together. Together they had made the revolutionary discovery of the global mid-ocean ridge system—the geological skeleton on which plate tectonics theory was hung. But Heezen chafed under Ewing's directorial authority and, in my opinion, began to ignore it. Growing animosity naturally led to a colossal confrontation. By the mid-1960s, two scientists who had collaborated magnificently weren't talking to each other. In 1967 Ewing reassigned to me many of the responsibilities relating to Lamont bathymetric data acquisitions, archives and data analyses, which had previously fallen in Heezen's domain. Having just gotten my Ph.D., it was not a pleasant situation to be in.

Loyalties were divided, stories abounded—many of which are badly distorted or wholly fabricated. Perhaps it serves little purpose to belabor the sad saga of conflict between these two great scientists, except to note that it was an unfortunate embarrassment to the observatory.

The Ewing-Heezen conflict was testimony that not everyone could or would work for Ewing. And that was certainly true for ship captains. For four years after Lamont acquired the *Vema*, Ewing and Worzel had hired captains who for one reason or another did not live up to their demanding standards. Then in 1957 they interviewed a Nova Scotian captain named Henry Kohler.

Kohler was a marvelous man, born to the sea, and he expected of his crew only very hard work, the desire to get the job done right, and complete loyalty. That explains why Ewing and Worzel hired him and why he remained captain of the *Vema* until the ship and man both retired in 1981. Kohler was a cornerstone of Lamont marine operations. He exemplified the Lamont philosophy that expected the

best that each had to offer, demanded that experiments be conducted around the clock in all conditions, and worshiped data. He also detested slackers of any flavor.

I first met Kohler that first summer I went to sea in 1961. He seemed a stern and demanding sort, unnecessarily tough, I thought. Clearly, a lot of people resented him because he had a very hard-nosed, old-school, the-captain-is-the-captain approach.

He had very fixed ideas about the way things should and shouldn't be done. He knew more about the ship and seamanship than anyone who came aboard and, over the years, he knew more about the mechanics of doing science aboard the ship than nearly all the scientists. So he wasn't terribly tolerant when he saw things being done in a way that he didn't think was right or safe.

Kohler contributed mightily to the continual efforts over the years, whenever money was available, to upgrade the *Vema* to make it a safer and more efficient working scientific ship. (In that campaign, *Vema* lost her elegant masts, her once-teak deck became steel, her white hull was painted black. But in her post-Lamont life, she has been restored to an elegant sailing ship and today makes luxury cruises in the Caribbean.)

He was rigid about schedules. Hell did have to freeze over for the ship to sail an hour late or to arrive an hour late into port. I have seen him cast off the ship with a crew member or a scientist running down the pier trying to get on, with the guy frantically getting some private boatman to take him out and get him on the ship. Kohler wouldn't wait five minutes.

He was just as firm and organized about everything else. Often he would say to scientists, "All right, we've got to head to port. It's going to take this much time and I've got to allow this much contingency and this much fuel." And that would often lead to arguments because the scientists would want to push the limits and squeeze every drop of science they could out of a cruise they had waited months to go on. Many scientists didn't like Kohler's authoritarian aura.

I, on the other hand, didn't mind, because I knew that he knew more than I did and that I could learn a lot from

him. In late 1965, just before my twenty-seventh birthday and a few months before completing my Ph.D., I was both thrilled and petrified to serve as chief scientist for two months on *Vema* 21. Taking charge early on was not so uncommon, especially for young scientists like myself who had already experienced many months of apprenticeship at sea.

When we started out, I told Kohler that I was very interested in learning more about the operation of the ship. He was skeptical. He told me that he had been out with many people who were gung-ho and revved up to learn. He had invested time with some of them, but inevitably after a few days, they became bored or preoccupied and rode off into the sunset. I said that wouldn't be the case with me. If he would spend the time and work with me, I would see it through to the end of the cruise.

During that long transit from Honolulu to Panama, we pushed the limits and ended up making a record number of stations and measurements. When I look back, all the time I was really pushing people, many of whom were older than I. I think Kohler was impressed by my energy and commitment, and gradually he took me under his wing. I did my work and, in addition, in the evenings and early-morning hours, I was up on the bridge every day for thirty-five days, "shooting stars" (taking celestial fixes) with the mates and the captain, learning about calibrating the ship's magnetic compass, reading a radar, and other facets of the ship's operation. After *Vema* docked in Panama, and before I departed, Kohler surprised me. He had taken the trouble of getting me a Panamanian third mate's license in recognition of my efforts to learn about the ship. I still have it.

Over many years and many months at sea together I learned to admire and respect Henry Kohler enormously. We built a cherished friendship as tight as could be between two people who don't share the same genes. He cultivated lifelong friends in exotic ports all over the world and he would write to them months in advance announcing that he would be coming. Whenever *Vema* came in, these friends would be delighted to host him and a select few officers, engineers, or scientists, like me, whom he liked.



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He put far more into his job than any captain I've ever known. He provided many apprenticeships for young seamen out of Nova Scotia, taking a lot of young boys to sea and bringing back men, and many stayed on with him. He trained them and looked after them, making sure they didn't blow all their earnings in each port. When they got off the ship after a year at sea, they found he had set aside a good wad of money for them.

Some people considered such tactics an imposition, but in the long run, I think everyone aboard the *Vema* was better off because of them. You could get angry with the attitude that the captain is always right, or you could conclude, as I did, that in Kohler's case, he usually was. In any situation at sea, there was no one I'd rather have as captain than Henry Kohler.

Well into his tenure as *Vema*'s captain, his two daughters had grown and left the nest. Set free, Kohler's wife, Laney, would occasionally join the *Vema* in some exotic port and to everyone's delight would sail on *Vema* for several months. Laney was a wonderfully warm and friendly person and everyone enjoyed her presence on board—not just because she was so nice, but also because when she was aboard, the good captain was definitely a notch more mellow, and we all benefited. Laney became my close friend as well and the three of us enjoyed many memorable parties along with Henry's global cadre of well-cultivated "business associates," in places no tourist has ever seen.

As our friendship grew, he would stretch the limits for me and do anything I would ask, if it was at all possible. He would say, "If that's what you want, if you really need this, if it's important, we'll do it."

At some point, my administrative experience and level of authority exceeded his. I became an associate director with oversight responsibility for the ship—his ship. He was tickled when he introduced me to his friends as his student and his boss. In the spring of 1997 Henry Kohler was selected to receive an honorary doctorate from Dalhousie University in Halifax for his work aboard *Vema* and for major contributions to the Canadian fishing industry. He died just weeks before the degree was to be conferred. Though he was never officially

pronounced Doctor Kohler, it really didn't matter—he had been selected, and justly so.

By the early 1960s, Lamont had grown accustomed to the way Kohler handled a ship's crew, so there were a few hitches in 1962 when we acquired our second major research vessel, the *R/V Robert D. Conrad*—the first of several AGOR-class ships built by the Navy exclusively for basic deepwater research by the academic community. Our halting beginning was caused mostly by our lack of experience and general disdain for unionized ship crews. But the Lamont work ethic gradually permeated the *Conrad*, which soon became the other Lamont world-ranging workhorse. Lamont scientists also had a near-monopoly on geophysical and physical oceanography programs on a third ship, *Eltanin*, a former cargo ship converted by the National Science Foundation to do research in Antarctic waters.

Also in 1962, the oceanography building was completed and I finally got a shared office. The new building, perched high atop the Palisades Cliffs, offended many, most notably the Rockefellers, who could see it from their estate across the Hudson River. To mollify them, the river side of this new light-yellow brick building was painted green and fast-growing saplings were planted. Today those saplings tower above the three-story oceanography building and compromise the previously magnificent view of the Hudson River. Scientists, students and staff of Lamont's Marine Geology and Geophysics (MG&G) group, along with the fledgling Physical Oceanography group, took occupancy and began to establish specialized computing facilities—an effort that was the natural extension of our credo and training to collect, process and archive data.

Soon we had acquired powerful IBM 1800 and 1130 computers, giving us data storage and display capabilities and computational possibilities that positioned us at the leading edge of this new technology. Manik Talwani spearheaded these initiatives, but many others, including Xavier LePichon, Walter Pitman, Ellen Herron, Jim Heirtzler and I, contributed to writing the requisite software that gave Lamont the unique capability to analyze vast quantities of data in digital formats.

It was clearly payoff time for all those years of relentlessly and systematically collecting global data aboard *Vema*, *Conrad* and *Eltanin*. We alone were sitting on the mother lode with all the tools needed to mine it. With the data and the means to analyze and synthesize them, we were perfectly positioned to test, validate, define and refine the simple, elegant but revolutionary theories of seafloor spreading and plate tectonics. There was an indescribable excitement in deciphering and synthesizing the marine geological and geophysical data taken by Lamont and others. Each day seemed to bring an important new discovery.

By the mid- to late 1960s, Lamont became the first institution to put computers on ships and start processing data at sea. We also accepted and processed data from the ships of other institutions that had not yet developed the capacity to analyze their own data using "modern" computing techniques. (Ironically, I now have access to more computer power on my desk than the entire observatory had during our heyday of discoveries in the mid-1960s!)

The Lamont system for processing geophysical data gradually permeated into the community, aided by our graduating students who carried the knowledge, philosophy and programs with them to their new jobs, like true disciples. The National Geophysical Data Center, de facto, gradually adopted our systems for archiving and transmitting data.

The mid-1960s were also golden times for research funding. Support was relatively plentiful for all of us. Lamont drew nearly fifty percent of its federal support each year from NSF and ONR. For a short time, it seemed as though the two were actually competing with each another for the privilege to support the most innovative and visible scientists here. We could spend the vast majority of our time actually doing science. Unlike now, only a very modest amount of time was required to "justify" it to funding agencies.

During this era, both ONR and NSF were funding Lamont's annual ship time and science projects "omnibus style," meaning we got block grants to be used and divided up as we saw fit. But in the late 1960s NSF ceased this practice—yielding to complaints and pressures from an

increasing number of other institutions that Lamont was getting favorable treatment. Nevertheless, Lamont had essentially blazed the trail in the field of marine geology and geophysics and continued to dominate it. For many years, Lamont routinely captured a third of all NSF's MG&G funding in peer-reviewed competitions. I often suggested to NSF program managers that they should just give us this share of the money each year, since the outcome of competitions was obvious!

Over time, ONR funding gradually shrank, as the Navy increasingly gave priority to projects that could clearly demonstrate a short-term relevance to naval operations. At the same time, NSF began funding new, large-scale programs such as the Deep Sea Drilling Program (DSDP) and the International Decade of Ocean Exploration (IDOE), in which Lamont scientists participated. At age thirty-one I had the privilege to serve as co-chief scientist on an early DSDP drilling leg off Morocco.

Deep-sea drilling began in 1968 with very little opportunity to do much prior geophysical reconnaissance to identify optimal drill targets (unlike today when surveys for drilling projects precede drilling by two to four years). Ewing decided to devote the 1968 *Vema* 26 cruise almost entirely to providing surveys to pinpoint drilling targets—not for the future, but almost for the moment. If all went well, we usually had a chance to bring the data back to Lamont for analysis and then collectively designate the best sites to address particular geological questions. But sometimes events overtook the best-laid plans.

As a young chief scientist on *Vema* 26, sailing from Recife, Brazil, to San Juan, Puerto Rico, I was given the assignment of collecting geophysical survey data on four potential sites for DSDP Leg 4 in the eastern Caribbean and western Atlantic. But the drilling had caught up with the surveying, and I recall working frantically aboard the *Vema* to synthesize our freshly collected data. Then we traveled by Zodiac several miles across open ocean to carry these scientific goodies to the DSDP drill ship, *Glomar Challenger*, where co-chief scientists Sam Gerard (of Lamont) and



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Dick Bader eagerly awaited them so that they could proceed with drilling holes on the Barracuda Ridge.

In the spirit of multi-tasking, we also accomplished some high-seas trade—recognizing, too, the hardships of going to sea and two other essential facts:

1) The *Glomar Challenger* was ostensibly a dry ship (no booze allowed).

2) The *Vema* had just embarked from Recife, a port not known for its quality meats.

We exacted a swap of vital resources. When the *Zodiac* headed back to *Vema*, we had left behind two cases of very old Scotch and we carried back with us about 100 pounds of prime beef, along with the inner satisfaction of knowing that we had “served science wisely and well” that day!

In 1972, a momentous event occurred. For twenty-three years, Ewing had fought a running battle with Columbia, usually over money and autonomy. On one hand, Lamont had grown into an enterprise far beyond a part of Columbia's Geology Department. It had raised most of its own money from federal and sometimes private funds, including the gift Ewing raised from the Doherty Foundation, which precipitated our name change in 1969. At the same time, Lamont was an integral part of Columbia, which owned the land on which the observatory was built. Administrative trench warfare persisted until Columbia gained a strategic advantage: Ewing was approaching 65, which at the time was a statutory retirement age for faculty. Ewing resigned from Columbia to become director of a newly created earth sciences research institute at the University of Texas in Galveston. He had no inclination to slow down and the position promised him greater prospects for more years of activity and leadership. But these weren't fully realized, because two years later, still going full tilt, Ewing died.

Manik Talwani was named Lamont's new director, succeeding Ewing, his mentor. He was an extraordinary scientist by anyone's standard, as well as a strong administrator. Like Ewing, he continued to pursue his own research while managing the observatory. His style and approach maintained and built on Ewing's undeniably successful foundation. The ships kept sailing and in December 1975,

south of Australia, I was chief scientist aboard the *Vema* when it became the first vessel to sail more than one million miles in pursuit of academic research. We were sailing in nasty latitudes, the roaring 50's, and as I recall, no one was in the mood for an elaborate celebration. We had commemorative patches made for the event and probably sucked down a couple of semi-illegal brews. Beyond that, it was hang on, keep dry, and keep that data rolling in.

But times were changing. In the late '70s, as the gold of the golden era of research funding began to fade, a small but powerful group of scientists at Lamont grew dissatisfied with Talwani as director. I cannot definitively say why because I was Talwani's associate director, and was conspicuously left out of the back-room discussions to remove him. Some felt federal funding agencies were favoring Talwani's particular research interests because of his position as director—a rationalization rather than reality, I believe. Others challenged his initiative to develop the infrastructure to take Lamont to the next generation of marine seismic research, multi-channel seismics—a decision that in retrospect I think was both bold and sound. Still others were just ready for a change in leadership style. Because of Talwani's imposing presence, they chose a covert pathway of dissent.

The undercurrent of discontent crescendoed before Talwani realized it or had any real opportunity to defend his actions. This lack of due process cast a pall on Lamont and spawned an unfamiliar distrust. It took some time for the resulting internal wounds to heal, and except for the departure of Ewing, no other event had a greater impact on Lamont than Talwani's “fall from leadership” here. During this time, I was on sabbatical at Stanford. As a ranking official in Talwani's cabinet, I was “asked” to resign my duties as associate director. It was perhaps symbolic that as Lamont was undergoing this upheaval, the *Vema* was also retired, and with her, Captain Kohler, a champion of both the ship and Talwani. These events happened fairly unceremoniously, in the midst of bad feelings and resentment across much of the laboratory. In my view, this was perhaps Lamont's most disquieting hour. Some of the extraordinary closeness of a

Lamont family, which had defined a unique essence of the place, was lost, never to be fully recovered.

Barry Raleigh was eventually named to succeed Talwani as the next director. He was from the U.S. Geological Survey, someone who did not grow up at Lamont, and in large part was brought on board in the role of "healer." Gradually the observatory's attentions were refocused on doing science. Not long afterward, I was asked to resume duties as associate director, including supervision of the ships.

In October 1985, the *Conrad* followed in the *Vema's* wake, becoming the second academic research vessel to sail one million miles. I had the good fortune to be on board, as I had on the *Vema*, and some people claimed I had arranged for this "coincidence." But even I couldn't really manipulate the logs or the computer-based records of miles sailed.

*Conrad* reached her historic milestone in the middle of the South China Sea. I was chief scientist and we were engaged in an improbable two-ship seismic experiment with a research vessel from the People's Republic of China. From the time we had conceived the project, it had taken nearly six years to write the proposal to do it, receive favorable peer review for it and finally implement it. But our Chinese colleagues had not grown up with the Lamont work doctrine, and when the weather turned foul, they opted to quit and head for the comfort and safety of a Chinese port, hundreds of miles from our research site. After several unsuccessful attempts to persuade the Chinese captain and chief scientist that we only had a limited amount of time to work and that it had taken six years just to get here, drastic action was required. I sent a radio message to the minister of the PRC Ministry of Mines and Resources (the equivalent of an undersecretary), asking him to intercede. Two days later the PRC vessel was back on location and there were no further altercations. My Chinese counterparts and I never spoke of this incident again, and the overall collaboration turned out to be an outstanding and scientifically rewarding one for all involved.

Around this same time, it became clear to me and others that ONR planned to retire aging AGOR vessels and to replace them with a lesser number of new vessels. *Conrad*

was among the first of the original AGORs built and was worked very hard, so it was targeted as an early retiree—even though the ship had been very well-maintained and had many years of service remaining.

Lamont was suddenly in jeopardy of being without a ship—a major threat to me and to everyone else who depended on ships to do our science. In 1987 ONR announced a competition to win a new vessel. The ante to enter the competition was an offer to retire an old AGOR. Knowing that the *Conrad's* days were numbered, we naturally submitted a proposal. The deadline to submit proposals to ONR was stern, and about ten days before it was due, I was stricken with a deep venal thrombosis. My calf was swollen like a balloon and I was hospitalized to prevent the clot from moving to a bad spot (my heart), to thin my blood and to manage the pain. After a day or two, the worst had passed, and I spent the next several days in the hospital working on the new ship proposal—with a steady stream of faithful support staff shuttling materials back and forth between Lamont and the hospital. The proposal was huge, but we won the battle and met the submission deadline. Unfortunately, about two months later I learned we had lost the war: The new ship was awarded to the University of Washington.

I never really knew if Washington's proposal was better than ours. We did learn, however, that the university had pledged \$500,000 of state support for ship operations each year and we felt, rightly or wrongly, that the new ship had been effectively bought out from under us.

Now ship matters were really beginning to look seriously bad. Typically, from concept and design to funding, construction and acceptance, acquiring a ship takes about six to eight years—if all goes well. As things turned out, not getting the Navy ship was probably a blessing in disguise, but I suffered quite a few months of anguish and heartburn before we created a new and even better opportunity to replace the *Conrad*.

We learned that Petro-Canada was putting a seismic vessel called *Bernier* on the market. It was only five years



old and had been used minimally. In a move reminiscent of Joe Worzel's dash to Nova Scotia to size up the *Vema*, several of us hopped on a plane to the Netherlands to get a firsthand look at the *Bernier*. After a few days of "kicking her tires," we were favorably impressed and devised a plan to bid for the ship and convert her to our purposes.

Because we had very little money and no endorsement from a federal funding agency or the scientific community, we initially looked for a possible partner. We turned to the University of Texas Institute of Geophysics (UTIG) in Austin, Texas, normally a Lamont competitor for academic seismic research. UTIG officials tentatively agreed to become a partner with us in this unorthodox venture. But they grew timid and withdrew when it became clear that substantial dollars would be at risk just to stay in the bidding game. We were on our own. Actually, it was even worse, because UTIG subsequently began an active campaign against our acquisition of the *Bernier*, presumably deducing that if we were successful, UTIG would be put at a decided disadvantage in competing for funding for future seismic research projects. UTIG failed to recognize or acknowledge that a state-of-the-art seismic platform operated by any academic institution could be used by scientists of every oceanographic institution, not just Lamont's.

The acquisition of the *Bernier* took on epic proportions. Our plan was to secure an option to buy by putting on the table a non-refundable deposit of \$500,000 from our own money. Then we would write a proposal to demonstrate why the ship was needed, how it would be modified, at what cost, and where required funds of about \$12 million would come from.

Desperate but undaunted, we approached Columbia's central administration and secured its commitment to give us a loan to cover the purchase cost. But the promise came with the clear understanding that we needed to get the NSF to agree to repay the university within seven years and that the university would also recover the debt service on its loan. All of these plans of high finance preceded a concrete proposal, any peer review or review by the NSF, and no readily identifiable government funds that could be directed

toward this most unorthodox procedure for rapid ship procurement (surely an oxymoron!).

Every one of these circumstances was so unusual that the odds of succeeding in overcoming any hurdle, let alone every one, were staggering. The university fronted the purchase cost. The NSF received and thoroughly reviewed our proposal (actually two painful iterations were required). The scientific community mostly supported our efforts. The NSF set new government speed records for implementing all the review steps, figuring out a repayment schedule, endorsing the proposal at the last minute, just in time for final review by the National Science Board. In December of 1988, I personally signed the papers that transferred \$6 million directly to Petro-Canada, which in return handed me the "keys" to the *Bernier*. A great weight was lifted from my shoulders—only to be replaced immediately by an even heavier weight: the responsibility of actually doing what we had glibly pronounced we could easily do in dozens of different meetings, hundreds of phone calls and thousands of faxes.

Less than eighteen months later, Lamont's new ship began her maiden voyage. That year and a half held plenty of personal anguish, heroic work by countless staff, successful litigation, and steadfast support from Columbia and from the NSF, but the ship came in on time and on budget. She was rechristened *R/V Maurice Ewing*. He no doubt would have been proud of our guts, innovation, dedication and efficiency.

Ironically, the University of Washington, which had won the competition with us for the new Navy AGOR ship, endured more than three years without the service of a major vessel, from the time its old ship was retired and replaced by the new one. We retired *R/V Conrad* in mid-1989 and were back in business with *R/V Ewing* by the spring of 1990. Like all her illustrious forebears, she has never slowed down, acquiring and processing data every mile on her way to achieving a million miles.

I plan to be there when it happens.

† From *The Flowers of the Sea*, Naval Institute Press



*"Some kids dream about running away and joining the circus.  
When I was a boy growing up in Rockland County,  
New York, the circus came to town infrequently—  
but Lamont was always there,  
seductively offering the chance of running away to sea."*





## A SEA CHANTEY

*Life in the Shop and on the Ship**by John Diebold*

*Seafarers often say that the only significant distinction between a sea story and a fairy tale is that the sea story lacks the traditional opening: "Once upon a time. . ."*

Some kids dream about running away and joining the circus. When I was a boy growing up in Rockland County, New York, the circus came to town infrequently—but Lamont was always there, seductively offering the chance of running away to sea. Over the years, half a dozen of my friends took low-level, unskilled, underpaid jobs at Lamont and disappeared—to return six months or a year later, occasionally tattooed, but always with sea stories (and usually swearing never to make the same mistake again). Eventually I took the plunge, too. I got a job in "the shop," expecting to be shipped out in a month or two. I hoped to join my pal Roy, who was sailing as cameraman on the *Vema* and had written letters encouraging me to come, with advice about getting a seagoing job at Lamont. My initial attempt, to work as a heat flow technician for James Hiertzler, failed to produce a job offer. Someone suggested I try the shop, and in early 1967, I did.

My job interview consisted of answering "yes" to three questions posed in a gravelly voice by Angelo Ludas, the legendary head of Lamont's machine shop:

"Do you like mechanical shit, young man?"

"Do you work on your own car, young man?"

"Do you want to go to sea, young man?"

A week later, I started work at an annual salary of \$3,200.

The shop was a busy place. Ten to twelve men worked in a crowded warren of sheds and trailers attached to the greenhouse of the original Lamont estate. Virtually all of the over-the-side equipment used aboard *Vema* and *Conrad* was fabricated there: everything from core heads, winches, cutting edges, water barrels and net frames to airguns, bottom cameras, strobe lights and pressure cases. The place was ruled with an iron hand by Angelo, who had started working at Columbia during the war (in the Manhattan part of the Manhattan Project) and was then taken on by Doc Ewing as one of Lamont's first employees. His first assignment was to find war-surplus machine tools and to set up the shop that would turn Doc's ideas into realities of steel. By the time I got there, a lot of other famous scientists were working with engineers and machinists, developing and prototyping things like heat flow apparatus and the lunar seismometer. It was an exciting place, but to me the most important part was that the shop supplied the manpower for the core crews, the essential three musketeers on each of two ships. Getting one of these jobs was my goal. It would be my ticket. My home would be the ship, the world my front yard. Somehow it didn't work out quite that way.

As the youngest, most junior (and virtually only) apprentice in the shop, I got stuck with the worst, dirtiest, unskilled jobs: chipping and scaling the rust from old gear housings returned from the ship; breaking off taps in corroded screw holes; painting with red lead and Rustoleum; welding,



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and eventually being permitted to run the oldest, clunkiest milling machine and lathe; turning out production runs of simple equipment items in mind-numbing numbers. Week after week, the ships stayed out, and nobody quit. As the months went by, I fretted in the shop, but slowly developed into a halfway competent mechanic and machinist.

Life in the shop offered a variety of jobs, experiences and rituals. For instance, coffee breaks were sacred. Standing at your machine a minute beyond ten in the morning or three in the afternoon was more than a departure from tradition; it was a sin that invited a glowering stare from Angelo. Coffee was shared in the small, sunny welding room. On my first day, having had no task yet assigned me, I was first into the room for the morning break. I looked around and selected the shabbiest, rattiest-looking, paint-spattered wooden box in view and sat on it. A moment later, I was saved from disaster by a co-worker, who sat down next to me and muttered indignantly: "That's Angelo's seat!" I jumped up and decided to stand around until everyone was settled and available territory was revealed.

As my first year went on, I started to work on airgun development projects. The airgun had replaced explosives as a repetitive seismic source in 1961, following an accident aboard *Vema*, that killed geophysicist John Hennion. Continuous seismic reflection profiling had been more or less invented at Lamont in the 1950s. The impulsive pressure of an exploding half-pound block of TNT would create a voltage in a hydrophone towed in the water behind the ship. The voltage would trip a circuit that started a metal drum rotating. The same pressure wave would travel downward, and some of its energy would reflect from the seafloor and deeper geological layers. When the hydrophone "felt" this returning energy, the resulting voltage would appear as a spark jumping between a sharp stylus and the metal drum and would be recorded as a black mark on the piece of paper attached to the slowly turning drum. The deeper the ocean, the more time required for the sound waves to travel down and back, and the farther down on the paper the mark would appear. As shooting progressed, the

stylus would slowly shift sideways, so that each shot left its own trail of smudgy black marks on the paper. When the paper was full, the trails of marks, like the scan lines on an old black-and-white TV screen, created a visible image of the seafloor and the underlying sedimentary layers. This system revolutionized marine geophysics and started a new era of sub-seafloor mapping that continues today.

The drums (at first adapted from commercial weatherfax machines and later manufactured in the Lamont shop) revolved once every ten seconds, but the detonation of each charge of TNT was the ultimate step in a continuous sequence of manual operations that took thirty seconds or more to carry out: Cut the fuse (twelve to eighteen inches); pluck a blasting cap from its waterproof, spark-resistant box; slip it on the fuse and crimp it; unpack a block of TNT (100 per wooden case); put the cap end of fuse into the block; fasten it in place with black friction tape; slip a fuse lighter over the free end of the fuse; pull the toggle lighting the fuse; and toss the smoking thing over the side, into the water. It was dangerous to try to keep up this pace twenty-four hours a day, much less go faster, and even at this rate, two-thirds of the possible data was missed.

The advent of the airgun solved this problem, and made things a lot cheaper—and safer, to boot. It took three shifts of "shooters" to keep up with the TNT operation, while one technician could maintain the airgun and the air compressor that fed it. One month of constant profiling required loading and storing twenty tons of TNT, while the compressors were driven with electricity created by the ship's generators. And the data came in faster and looked better.

The airgun we used had been designed and developed at Lamont, much of the inspiration and perspiration coming from John Ewing, Doc's younger brother, and Roger Zaunere. The two had invented a simple but effective airgun in the mid-1960s, and its design was continually being modified and improved.

Now a new era in Lamont's marine operations began. Each of two Lamont ships, *Vema* and *Conrad*, started a program of continuous geophysical surveying, circling the globe,

collecting data on each of up to 320 days a year. At least once a day, the ship would stop "on station" to take sediment samples and bottom photographs. The rest of the time was spent cruising at ten knots, trailing magnetometer, airgun and hydrophones, and measuring changes in the Earth's gravity field with the gravimeter mounted on a gimbalized, gyroscope-stabilized table in the belly of the ship. Instead of returning home after a "cruise" of three or four months, the ships stayed out longer and longer. In late 1966, the *Conrad* was about to return home, finally completing its first year-long cruise, RC111. And at long last I was sent out to join the boat in Panama, *Conrad's* penultimate port. My task was to get on-the-job training as an airgun man, apprenticing under David Crippen.

I flew to Panama in October to meet two youthful chief scientists, Bill Ryan and Steve Eittrem, and a jaded crew, eleven months out. Crippen was gruff but competent, and I learned what I had to know. As we sailed into the Caribbean, the ship started to roll, and I had to deal with the green-hued demon of seasickness. The queasiness was worst when I was below decks, and something told me to go out and up. I spent an hour on the flying bridge, hanging on to the railing, eyes fixed on the horizon, training my brain to associate what my body sensed with the ship's actual rolling motion. For some reason, this cured the seasickness. I am blessed that it never came back.

The principal amusement during that leg consisted of catching sharks while the ship was on station. Sharks are hard to pull in but easy to catch—it doesn't take much more than pandering to their instinctive greed and using a big hook. I was struck by the bloodlust that affected nearly everyone exposed to the scary presence of a wildly flopping eating machine on deck. Sharks are hard to kill, I learned, and they smell bad when cut open. Several had their jaws cut out for trophies. Hung out to dry, the exposed razor-sharp teeth still presented a hazard to the unwary. In one case, a snapping corkscrew roll set everyone on deck reaching for something solid. One poor fisherman grabbed a drying jaw instead of a hatch dog. Sliced to the bone by a dead shark!

We pulled into the pier in Piermont in early November, and after a few days off I went back to the shop to help get the ship and its gear ready for the next cruise, for which I'd be the airgunner. On my first day back, Angelo detailed me to work with Joe Worzel, associate director of Lamont and Doc Ewing's right-hand man. Doc had a reputation for getting things done with relentless hard work, and Joe seemed to be trying to outdo him. Nothing I could do seemed to please the man, and I fumed under what I felt to be unjustly contemptuous criticism of every effort I made. During the lunchtime break, I warned Angelo that I was ready to kill Dr. Worzel. Ange couldn't stifle a little grin—wordless confirmation that he knew what I was feeling and why. I was reassigned on the spot.

The *Conrad* had been out for a year, and there was a lot of gear to repair, refurbish, replace and upgrade. From my angle, important projects included improvements of facilities and furniture in the ship's machine shop, which was to be my domain, and the installation of two new air compressors. Little did I realize how much time I'd be spending maintaining, repairing, modifying and generally babying those two demanding machines!

Meanwhile, back on the Lamont campus, final touches were being put on a new, extra-long hydrophone array being built in Harry Van Santford's electronic shop. Back in the TNT-profiling era, a few sensitive hydrophones were towed behind the ship to detect the reflected sound waves. Since dragging the bulbous phones through the water at ten knots made signal-obscuring noise (gurgling and swooshing sounds), a special winch was constructed that paid out the towing wire at ten knots for a few seconds at a time, stopping the hydrophones' forward progress while the reflections were coming in. Between shots, the winch would take up the slack, pulling the phones through the water at double speed. When the introduction of airguns allowed a threefold increase in shot repetition rate, there wasn't enough time for slacking the hydrophones, and a new method had to be developed.

The answer was to use a longer string of smaller hydrophones, strung like beads inside a piece of flexible tubing



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filled with buoyant, lighter-than-water oil. The resulting "streamer" was pulled through the water at constant speed by the ship, but early versions were still found to be noisier than the original slacked hydrophones. The extra-long streamer was created in an effort to improve the signal-to-noise ratio by adding more hydrophones, and increasing the overall length of the array.

I had a few other projects under way, too. Selling my car, storing my books and records, trying to get my teeth fixed up in marathon dental sessions, getting fitted for contact lenses, and slogging through the Coast Guard's bureaucracy to get my seaman's credentials—the prized "Z" card that promised casual employment any time the holder was willing to go to sea in the merchant fleet. Winter was coming, too, and as every day went by, the winds blew a little colder down the Hudson River Valley, chilling workers out at the end of the pier. Small flasks of brandy began to bulge in coverall pockets. To my surprise, the schedule was advanced, and we sailed from Piermont three days early—too soon for me to complete either my Z card or my dentistry. The latter deficiency was to have effects that are felt even today.

RC1201, the first leg of *Conrad's* twelfth "cruise," started on January 6, 1968. The ship left Piermont with eighteen inches of deepening snow on deck. As we left New York Harbor, the wind picked up and the storm evolved into a snowing gale. The ship started rolling heavily, and heavy objects, hastily stowed in the machine shop and lazarette, started to shift, work loose and eventually slide around, making those enclosed spaces a hazardous environment. I'd been worried about getting seasick, but I wound up working for forty-eight hours straight, stowing the loose gear and metal stock and tying things down. The storm continued, but I was too busy and tired to get seasick. The ship was doing its job, carrying us southward until the weather finally calmed down, but then it seemed as if everything in the scientific department was breaking down. The new, long streamer developed electrical problems. One of my new compressors failed, with a broken cylinder.

Six days out, we pulled in close to a Florida port and exchanged one seasick electronics technician for compressor parts, brought out by small boat.

We continued southward, and slowly the scientific operation was brought into workable order. Each day's effort typically was shared between the gathering of geological samples and geophysical data. The work went on twenty-four hours a day, seven days a week, typically for a thirty-day cruise. Most of the time was spent motoring at ten knots, collecting continuous geophysical measurements; but at least once every day, the ship stopped for a coring and seafloor camera "station."

Underway measurements included gravity, magnetics, bathymetry, single-channel seismic profiles and seawater temperature. Bathymetry, or water depth, was determined by the time it took for a short, high-frequency pulse, or "ping," to travel from a hull-mounted transducer to the seafloor and back. Two "pingers" were typically used: the high-frequency one (12 kilohertz) gave the most accurate results, but the 3.5-KHz pings had the capability of penetrating soft sediments and were very useful in deciding where to take cores. The gravimeter was installed down in the engine room, as close to the center of the ship as possible. Measuring the force of gravity is conceptually simple: You determine the apparent weight of some reference mass (or, equivalently, the speed with which it falls when dropped). But this is not so simple when the measurements are made on a constantly moving platform. Think of the sudden lightness you feel as an elevator begins to descend and the knee-bending heaviness when it stops at the ground floor. And a ship is not only moving up and down, it's constantly turning, pitching and rolling. Think of being thrown to one side as your taxi driver makes a sudden, unexpected turn. These confusing effects never disappear on a ship, but they can be minimized by placing the gravimeter as close as possible to the ship's natural axes of motion.

Measuring the local strength of Earth's magnetic field was a bit simpler. The protons in hydrogen atoms act

like little compass needles: They wobble, or "precess," around an axis that is in the direction of the Earth's magnetic field. Our magnetic sensors were heavy plastic cylinders filled with hydrogen-rich oil, surrounded by an electric coil. When the coil was energized, it created a powerful magnetic field inside the oil-filled cylinder, and the proton-compass needles would briefly realign themselves with this field. When the current was turned off, the protons were free to "precess" in realignment once again with the Earth's magnetic field. The speed of this procession was measurable and depended on the strength of the Earth's magnetic field at the particular location where the measurement was taken. Hence the local strength of the Earth's magnetic field could be determined. Once the electronics were worked out and built, this was a relatively easy measurement to make, but the magnetometer had to be placed far away from the ship, whose steel hull distorted the ambient magnetic field. To accomplish this, the sensor bottle, usually referred to as the "maggie," was towed at the end of a long, heavy wire. The shipboard end was connected to the magnetometer's electronics and chart recorder.

The seismic profiling gear included two items—the acoustic source and receiver—both of which were towed behind the ship along with the magnetometer. The acoustic source was usually called an "airgun," though this nomenclature could cause problems when shipping the things. To keep from alarming customs officials around the world, descriptions like "pneumatic sound source" were often substituted.

Nowadays, more advanced airguns are essential to offshore exploration for oil and gas, and thousands are in operation every day. In the mid-'60s, however, the exploration industry was just starting to make the transition from explosives to airguns, and it was almost impossible to buy them, so at the time, the Lamont gun, designed and manufactured in the Lamont shop, represented the state of the art. It was primitive, but at the same time elegant in its simplicity and efficiency. It had only one moving part, and its operating principle was the same as that in every airgun

made ever since. Air, compressed by a factor of nearly 150 to a pressure of 2,000 pounds per square inch, was pumped into a storage chamber in the gun. A kind of plug (usually called a "shuttle") held the air in the chamber. Some of the air was diverted and used to hold, or "control," the shuttle in place as the pressure built up. In modern airguns, the gun is fired when an electrical impulse activates a solenoid valve, shifting the "control" air from holding the shuttle to freeing it, letting the larger volume of pent-up air escape from the chamber into the surrounding water. The Lamont airgun differed in that it went off when the air pressure reached a certain preset level. This arrangement eliminated the complexity and expense of a solenoid valve, and although the exact moment of the shot was a bit unpredictable, the recording system (originally designed to be triggered by even-less-predictable TNT detonations) handled things flawlessly.

In preparation for a station, the chief scientist would ask that the ship slow down to two or three knots, reducing the towing strain. The trailing equipment would be pulled in, always in the same order, and by hand. First, the maggie was recovered by two scientists who shouldered the dripping, thick and heavy cable at the transom and walked it forward in turn, flaking it on deck, making the "figure eight" pattern that prevented tangling when the gear was redeployed. Next, the airgun tow rope was pulled in, using one of the ship's capstans this time, though the trailing air hoses had to be pulled in by hand. Finally, the most exhausting item, the seismic streamer. Connected by a thinner wire, the streamer offered a lot more towing resistance than the maggie did, and it was necessary for two or three people to team up, straddle it and pull it in, hand over hand, as yet another scientist flaked it in a safe place along the port rail. The sight of the streamer itself, finally emerging from the water, always brought a sigh of relief, as it signaled the end of the ordeal. This exercised our forearm muscles so much, I began to realize why this feature had been so prominent in the caricature of Popeye the Sailor Man.



106 | With the geophysical gear on deck, the ship could be stopped at the desired spot. A spectrum of new station-related activities began on deck. A "full" station included sediment sampling, heat flow measurements, bathythermograph, bottom photography, current measurements, nephelometer profile and biological sampling. "Lamont style" was to do as many of these things as possible—simultaneously. This involved having as many as five sampling assemblies dangling from three different wires, all at the same time. It was routine on Lamont ships, but (I later learned) was absolutely unheard of on ships operated by other oceanographic institutions. This strategy saved a lot of time, but it could be risky and had to be carried out with skill and care. First, the seafloor camera was launched and lowered from a middle-sized winch situated on the "01" deck, one level above the main working deck. During RC12, the camera apparatus was evolving from the simple "sled" originally designed by Ed Thorndyke, to a tripod, which could house the bottom camera, its strobe light, the constantly measuring nephelometer (which measured the water's "cloudiness" caused by suspended particles), and a current meter as well. Light in weight, large in cross-section, the camera apparatus tended to move with the currents, "kiting" away from the ship, which was being held by the officer-on-watch so that the camera wire was on the upwind side.

Once the camera wire had established a stable orientation with respect to the ship, the coring apparatus, which had been previously set up by the coring crew, was hoisted from its cradle, swung to its vertical position, and lowered by the diesel-powered coring winch. The coring apparatus consisted of a 1,800-pound core head, connected to the wire by a trip-arm assembly, which released it to free-fall twenty feet or so when it approached the bottom. Attached to the core head was one or more twenty-foot-long sections of pipe, which were driven into the sediments by the inertia of the free-falling head.

The core head provided more than mass, however; it included tubes designed to hold more subtle devices. A fully loaded core head included a strobe light and a camera,

whose function was to photograph a compass, bolted to the pipe below, establishing the orientation of the recovered sediments. A third tube was filled by the heat flow recorder, wired to a series of sensors placed along the length of the core pipes. After plunging into the sediments, the corer was allowed to rest in place for ten minutes or so as the sediments cooled down from the slight frictional heating produced by the pipe's penetration, and thermal equilibrium was established. By accurately measuring the temperature at two or more depths (and knowing the thermal conductivity of the sediments, which was measured later, from the cored sediments themselves), the amount of heat being given off by the Earth's crust could be determined.

Sometimes, water sampling bottles would be attached, either to the core or camera wires. These were sent down open, with the seawater flowing through them. When they reached the desired depth, a heavy brass "messenger" was clamped over the wire and dropped, to slide along the wire and trigger the closing of the sampler's lid, trapping the water inside for later geochemical sampling. The largest of these samplers was the 250-liter "Gerard barrel," named after its designer, Robert "Sam" Gerard, one of Lamont's early scientist-engineers. The third wire over the side reeled off the smallest, "BT" winch. On its end was either the eponymous bathythermograph meter, used to measure water temperatures at various depths, or a biological sampling net, lowered last, and taking the least time.

If all went well, the three wires would be recovered in reverse order; BT, core, camera. Of course, things didn't always go so well. The downside of two- and three-wire stations was that the wires could get tangled. This was especially likely to happen when the water barrels were used, or in the presence of undiscovered deep currents running in unknown directions, which could drag the camera back toward the core wire. Tangling was bad for the wires themselves, since the thinner camera wire could actually saw through the half-inch core wire, unless the two were reeled in at the same speed. Most of the time, the wires were untangled after careful removal of the water barrels,

and a lot of sweating and swearing. Once in a great while, a wire had to be tied off, and then cut, so that things could get straightened out. After the cut wire had been temporarily reattached and the equipment retrieved, the bo's'n would have a day's work in front of him, long-splicing the wire again.

A more frequent mishap was a bent core pipe. Predicting how far the falling coring apparatus would penetrate into the bottom was an uncertain business, especially in a new, previously unsampled area. The desire to recover as much sediment as possible often led the chief scientist to an over-optimistic selection of pipe length. If the giant dart comprising the core head and pipes didn't strike the bottom exactly vertically, pipes would bend. Upon recovery, the first problem was how to land the suddenly V-shaped core pipes into cradles designed for straight pipes. The pipes, sticking out at odd angles to the ship, had to be disconnected, and then straightened, using a portable hydraulic press, before the sediments could finally be extruded.

As soon as the core head and camera were aboard, the ship would get under way, the geophysical gear redeployed, and the cycle would start again. To do all this took ten or more scientific personnel. Recording and annotating the geophysical data required watchstanders, who (while the ship was under way) worked at these tasks between eight and twelve hours a day. The core crew and airgun man were exempt from standing watch, since they had so much else to do. This exemption usually extended to the core describer as well, since he had to spend so much time sampling, describing and archiving the core samples. The ET, or electronic technician, usually had more work than he could handle, too, as did the cameraman, who spent so much time in his darkroom and behind the camera winch. Less favored were the heat flow, gravity and maggie men. When we were lucky, the chief scientist brought along some students who could assist; otherwise those guys had to help out.

On the other hand, there wasn't much else to do during what little leisure time we did have. In those days,

there were no videotapes, VCRs or even televisions on the ship. No movies, either, and microwave popcorn hadn't been invented yet. Mostly, the entertainment choices were eat, sleep, read or work—and reading while on watch was a major no-no. There was no formal library, but rather the usual collection of dog-eared, left-behind paperbacks and books donated by various seamen's missions around the world. A lot of the time, we (the scientific crew) devised our own entertainment. During one leg, we made kites and tried to fly them from the fantail with varying degrees of success. I remember another project that met with even less success—trying to make plywood boomerangs. Since none of the dozens we made ever returned to the ship, each was a one-toss wonder. A more frequent sport was our own version of the national pastime, which we played on the fantail and called “tape ball.” Making the ball out of rags and friction tape was a practical and economic necessity, since the ball was prone to being lost, especially if anyone got a good hit with the mop handle we used as a bat.

Nighttime entertainment included playing cards, though seldom did anyone have much money for poker. Frequently, therefore, we used matchsticks and other counters and played for “payday stakes,” in which case accounts were settled when cash became available in the pre-port salary draw. Losing steadily could result in a financial shock, and not much fun in port. I played payday stakes for about three months running. After a series of highs and lows—alternately accumulating winnings and losses of as much as a hundred dollars—I broke even, and decided my nerves couldn't take the stress. Consumption of alcohol was also a favored nighttime diversion. Even before today's universal policy of running dry research vessels, Lamont captains ran tight, disciplined ships. But as my ex-boss, ex-advisor and longtime colleague John Ewing put it: “It's better to sail on a dry ship than one with no alcohol.” Even Captain Bligh on the *HMS Bounty* understood the limits of prohibition, the power of small favors, and the thin difference separating “deprived” and “depraved.” The trouble, of course, is that there were no



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liquor stores at sea, and it seemed that after a week or two the booze always ran out.

Less frequent but much more public occasions for sport and amusement were shipboard rituals, including boat drills and equator crossings. Coast Guard regulations mandate weekly safety drills, intended to keep sailors and scientists familiar with emergency duties and procedures. When the weather was good, *Conrad's* two clumsy steel lifeboats were lowered into the water on each side of the ship, amid a lot of teamwork, shouting and gesticulation. Eight scientists boarded one boat and eight ship's crew boarded the other. After being dropped into the water, the crews cast off, put out their oars and raced one lap around the ship. After eight months or so at sea, the scientists were able, at least once in a while, to beat the crew.

Equator crossings were marked by a ceremony that has been played out at sea in similar circumstances for hundreds of years. The shellbacks, who have crossed the line before, take charge from the polliwogs, who haven't. On *Conrad*, what followed was essentially a hazing ritual, but it retained resonances of the original, mystical obeisances to the powers of the deep. The polliwogs were sequestered, nervously discussing their anticipated fate, while things were made ready on deck. Eventually, the fun began. One at a time, the candidates were blindfolded, led out on deck, and (at least in my case) fettered with chains. On hands and knees, the supplicant crawled through a gauntlet of abuse, to be smeared with grease, paint and foul-smelling fluids. A tugging accompanied by snipping sounds convinced us that our hair was being cut (a serious threat to a young man in the 60s), though it turned out later that this was just an illusion induced by cutting the bristles of an old paintbrush close to the paranoid sufferer's ear. At the end of this avenue of indignities, the blindfold was removed, and the polliwog faced the royal family, usually consisting of King Neptune, his wife and baby, all dressed in appropriately fantastic style. This was, essentially, a kangaroo court. Accused of imaginary but always appropriate and funny sins against the empire of the sea, the

polliwog was given little chance to defend himself. (In the full-blown version, there's a judge, a priest, and a hopelessly incompetent public defender.) Punishment, typically involving a seawater bath, was then meted out, and on they went to the next miscreant, as the ex-polliwog, pleased and relieved, joined to watch the fun.

The most fun of all, anticipated and discussed from the first day of every leg, came during port calls, usually three days following a thirty-day leg at sea. Three days wasn't really long enough to dissipate the tensions built up after more than four weeks of continuous workdays, but we tried—and unlike the ship's crew, scientists didn't have to stand watch in port. As long as the work got done, time management was up to you. The ports of RC12 were pretty good: Panama, Manzanillo, Honolulu, Brisbane, Tokyo, Adak (oh, well), Kodiak (a little better), Suva (yippee!), Madrid del Plata, Buenos Aires, Cape Town, Colombo, Sasebo, Hakodate. Each place offered new adventures and new things to see, taste and experience. Youth and lack of responsibility were useful attributes. After the gear was pulled in for the last time on each leg, I worked feverishly to overhaul my compressors and guns as we steamed into port, trying to get all my work done in advance. Channel fever might have kept me awake most of the last night at sea, and I was often a sweating, greasy mess as we entered the harbor, but youthful energy and the prospect of something new pulled me through every time.

One of the last and most important ceremonies of each leg was the "draw," when the ship's crew and the long-term technical staff lined up outside the captain's office to receive some cash, drawn from our salaries. Typically, the request for this had been made days in advance, and it was important to predict accurately how much would be required. I learned that \$100 per port day was a good figure, unless there was something big to buy, like cameras in Tokyo. That seemed like a lot of money then, and it was, considering that my regular salary came to a lot less than that, but there were things to do, beer to drink, exotic food to eat, fun to be had, and it just wouldn't do to be

unprepared. Wine, women and song. Well, perhaps it's best to skip over the "women" part a little, but there were no women on research ships in those days, except once in a while the captain's wife. And in every port (well, not Adak) establishments could be found where socially deprived seamen could meet girls, talk and dance. I'll never forget what happened while I was sitting drinking beer with a young lady in a place in the outskirts of Colon, near the Panama Canal Zone. She asked, "What ship?" and when I said "*Conrad*," she giggled and ran back to her room. She emerged, shuffling a thick stack of Polaroid pictures, and came up with one of her sitting on the laps of two Lamont colleagues. The world my front yard, indeed!

Small though it was, *Conrad's* crew fell into a well-defined social structure. This was clearly visible in the dining arrangements. The galley was small, but the dining area was divided into three zones: officers, scientists and crew. Officers and scientists were served separately by a messman; the crew lined up at a passthrough window into the galley and were served directly by the steward. I was told that the officers/scientists mess had originally been integrated, but that the scientists' dining behavior had offended the officers to the extent that a partition was erected. I'm sure this is true, because the hijinks and food fights we engaged in then would be offensive to me now. Among scientists, there was a fairly clear division between short-timers and, as we thought of ourselves, "lifers." Chief scientists and the students they brought with them typically sailed for one or two legs, after which they went home again, with their data under their arms. On the other hand, the technicians were expected to spend a long time continuously at sea. Six months was generally considered to be the minimum. In retrospect, the enforcement of this minimum probably wasn't legal, but enforced it was. A fifty percent salary bonus ("sea pay") was withheld if you "made waves" and quit early. In some cases, malcontents were reported to have found that the cost of their return tickets was deducted from their pay.

As the months rolled by, RC12 got longer and longer, and the technical crew became somewhat jaded and

battle-weary. Chief scientists came and went, each full of energy and wanting things to be done in some particular way. We tried to make adjustments, but we thought we were doing a pretty good job already, and to be told "They do it differently on the *Vema*" rankled a slight but sensitive inferiority complex. On the other hand, it must have been frustrating for the chief scientists to have to deal, one at a time, with each member of this recalcitrant crew. From time to time, however, an interesting project, a chief scientist with a compelling personality, or a near-disaster, mechanical or natural, would come along and totally re-engage our attention.

Seismic projects that involved "shooting" explosives captured everyone's attention. The airguns were fine for reflection profiling, but to determine deeper structure, like the depth of the crust-mantle transition (the Mohorovic discontinuity, or Moho), required more power. The first step, loading the explosives onto the ship, was usually the hardest part. Typically, the U.S. Navy supplied us with TNT, or tetrytol, from the vast World War II surplus stored at various facilities around the Pacific. Navy stevedores would bring the stuff from the storage bunkers and leave it on the dock, alongside the ship. From there, it was our job to stow it safely. The bo's'n and his seamen would move the wooden boxes, fifty or seventy-five pounds each, onto the fantail, but the rest of the shipboard handling was the responsibility of the scientific staff, which usually meant the core crew and airgun man. One by one, the boxes were lowered through small hatches two decks below into *Conrad's* magazine, located underneath my machine shop. Fully packed, the magazine could hold about seventeen tons. When it was hot, which it usually was, working in that stifling airless space with its low overhead and tight access was hell. We took turns, so nobody had to work down there more than fifteen or thirty minutes at a time.

When the shooting started, the boxes had to be hoisted out again, but at a more leisurely pace: five or six every hour. Making up the charges and setting them off was the job of the chief scientist and one other person, who



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acted as his relief. Apparently by tradition, the airgun man was usually the second shooter. Chief scientists Bill Ludwig and John Ewing taught me the strict rules we followed, in the successful hope of preventing another accident like the one that killed John Hennion.

I have never found another task that held my attention quite the way that cutting fuse, crimping blasting caps, strapping together blocks of high explosives, lighting the fuse and dropping the charges over the rail did. Timing was always important. The "listener"—usually another ship on station with hydrophones dangling in the water—turned on its analog recorders for specified periods and at specified times, when the shot was expected to go off. We had to predict how long it would take for the fuse to burn, and have the shot ready to go that far ahead of time.

In Sasebo, a port on the southern island of Japan, Doc Ewing came aboard as chief scientist for a single leg during my tenure on *Conrad*. When it came to strong personalities, few could compare with Doc. I think we worked harder during that voyage than any other, which reflects Doc's constant desire to get as much data as possible at all times. The plan was to carry out two-ship refraction surveys in cooperation with a Japanese ship. *Conrad* was already loaded, as usual, with tons of TNT, but Doc had failed, somehow, to obtain blasting caps. Unfazed, he handed me a box of .45 pistol bullets as we left the harbor: "See if you can figure out a way to detonate the TNT." Meanwhile, he set the core crew to take three cores a day, every day, in the shallow waters of the Japan Sea. I worked on the TNT problem for a day or two. It was hopeless. Doc agreed. He told me to give up and help the core crew, instead. With two two-man crews, we were able to meet Doc's insatiable demand for cores, but I've never been so relieved to see a chief scientist leave the ship.

Not too much later, we arrived in Honolulu, the final port of RC12. It was September 3, 1969, and I had been aboard for twenty months. Time to go home. . .

. . . Exactly two decades later, *Conrad* once again left the pier in Piermont. The co-chief scientist for the cruise

was one John Diebold, who by then had earned his Ph.D. in geophysics from Columbia in January 1980. This time the ship sailed up the Hudson, making the first-ever attempt at a seismic profile of an inland river system. It was *Conrad's* last research cruise. After more than a million miles of oceanographic research, the ship was still breaking new ground.



DEC 1962

*"Georg Wüst's mission was to teach all of those  
Lamont geologists something about that  
'murky mist that keeps me from seeing the bottom,'  
as Ewing once said."*





# THE BIG BATHTUB

## Charting the Circulation of the World's Oceans

by Arnold L. Gordon

Doc Ewing's dogged determination and relentless energy set the pace for Lamont to take on and accomplish ambitious research endeavors that others had thought too daunting to attempt. But as the story goes, even Ewing couldn't find the plug to drain the water that obscured his clear view of the seafloor. So he decided that we had better learn more about what all that water was doing.

Doc was more interested in studying the sediments on the seafloor. But what he needed, and he knew it, was an understanding of all the processes that take place within the ocean that move sediments around and help shape how they lie on the seafloor.

In February 1961 when I first visited Lamont, I didn't know I was part of a grand scheme to bring physical oceanography to the observatory. Ewing had engineered a grant from the Ford Foundation that allowed Lamont to hire a visiting professor: Georg Wüst, who had recently retired as director of Institut für Meereskunde, the great oceanographic institute in Kiel, Germany. Wüst's mission was to teach all of those Lamont geologists something about that "murky mist that keeps me from seeing the bottom," as Ewing once said. The Ford Foundation grant also provided a full fellowship for a graduate student, who turned out to be me.

The ocean is immense, and studying it has always been difficult and expensive. Expeditions to take measurements of the ocean had been few and far between, but Wüst had participated in one of the more legendary ones. As a young scientist, he was aboard the German research ship *Meteor*, which between

1925 and 1927 crisscrossed the South Atlantic, methodically measuring ocean temperature, salinity and oxygen content from top to bottom. The cruise was nearly aborted when the ship's original chief scientist died off the coast of Argentina, but Wüst was put in charge and saw the expedition to completion.

But even the *Meteor* covered only one small part of one small ocean. There is three times more ocean than there is land on the planet—and the area to explore is even greater when you consider the vast depths *beneath* the ocean surface. In 1961, oceanography was still a young science. On a basic level, we knew that the global ocean wasn't the world's biggest bathtub filled with lots of water just passively filling in depressions on Earth's surface. But we had only a rudimentary understanding of the complex processes by which waters circulate around the globe and from sea surface to seafloor. Not to mention what today has become the fundamental quest of physical oceanographers: understanding the ocean's inextricable links with Earth's atmosphere and its critical role in governing the planet's climate.

In my mind, the early 1960s seem only a short while ago, though 1999 is as far away from then as the '60s were from the mid-1920s, when Professor Wüst was in his prime, in the midst of his epic survey of the South Atlantic aboard the *Meteor*. On the day I first came to Lamont in 1961, I drove the family car, a 1957 Plymouth, from my house in Brooklyn, New York, accompanied by my father, who complained about my wide turn from Route 9W onto Washington Road, toward Sneden's Landing and what was then the main entrance to the observatory. I was eager to explore.



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Ever since I was a boy, I had had a strong interest in natural phenomena. I used to track storms across the country, using National Weather Service maps. In college, for a geography class, I wrote a paper contemplating why Greenland and Scandinavia had such different climates, though they lay on the same latitude. So on that February day while my father took care of the family dog, separating her from a fight with Ewing's dog Erebus (named after a legendary oceanographic vessel), I was asking Jack Nafe in his small office in Lamont Hall if it was really possible to study physical oceanography at Lamont. Persuaded that it was, I signed up and was instantly swept up by the atmosphere of exploration and adventure that permeated Lamont. I became steeped in the philosophies and practices that guided its research.

From the start, Lamont has been called not an institution, but an observatory—for good reason. The Lamont tradition, instilled in all of us by Ewing, was to go out and make observations and measurements, to collect ravenously all kinds of data from all over the Earth. It was fine to debate various theories about how the Earth worked, but data spoke for themselves—and loudly—providing the empirical evidence to prove or disprove theories, and the essential ingredients to create and pursue new ones.

Ewing was an imposing figure. I remember walking the long hallway to Doc's office on the second floor of Lamont Hall, the hallway that seemed to grow longer and longer as you walked down it. I remember seeing photos of Doc grabbing some well-deserved sleep on the deck of the *Vema*, himself coiled up in the coils of the long hydrophone eel used to record seismic reflections of the seafloor. I remember the extraordinary clutter on his large table—those ever-mounting piles of data that needed to be examined.

From Ewing emanated the Lamont personality: a Spartan life of hard work and dedication; a career driven by an intense need to explore; and a certain attitude of not giving in to the idea that you couldn't go someplace or do something, in the quest for clues to understand the Earth.

Lamont back then seemed like an outpost looking out on exciting frontier territory. With so little known

about the Earth, every measurement made had the potential to uncover something new. Every new instrument developed in Lamont's machine shop could reveal new information. The instruments were designed to make observations easier, quantity being more important than high precision. While other oceanographic labs in the 1950s and 1960s tended to concentrate on a specific region of the ocean, the Lamont scientists were on a crusade to pursue observations from all over the globe—a perspective that was unique and ahead of its time. Lamont collected pieces of the puzzle from all over in an effort to put together the big picture.

The philosophy of Ewing and Wüst was that you learn by doing: You go out on ships and you collect data and work with them. You are an apprentice, working not only with a great scientist who is inspiring and who knows a great deal, but also with other students, perhaps a few years ahead of you, whom you could observe, brainstorm with, and also learn a lot from.

So, along with every other graduate student, I fell into the Lamont style of working weekends and late into the night, driven by the quest to know, to divulge something new, poring over the few data points available, making maps by hand. And as I pieced together data to decipher the dynamics of an entire ocean, I always kept in mind that the ocean I was working on was just one part of the global ocean.

Thus, Wüst assigned me my first apprentice's task: the challenge of studying the circulation of my first body of water, the Caribbean Sea, based on data that had been collected, but not yet analyzed, during the International Geophysical Year of 1957-58. It became my doctoral thesis.

In the early stages of that research, I remember the hours I spent in making maps of the Caribbean's oceanography. Wüst would pass by my desk, glance at my work and then point to a bad data value or, more likely, some poorly contoured segment in my map. It was a sign of his experience, developed over so many years of examining individual data points and extracting knowledge from them, and of his hard-earned innate sense of "how the ocean worked." Wüst knew that budding oceanographers needed to develop their own ideas of "how the ocean works," building on their

own experiences, which included making some mistakes and having the daring to explore new vistas.

The next step of my apprenticeship required field experience, and in 1963 Wüst arranged my first seagoing expedition, with Arthur "Rocky" Miller of the Woods Hole Oceanographic Institution aboard WHOI's *Atlantis II* to the Indian Ocean. This is where I first learned about fieldwork, how difficult it is to get each precious bit of data. Immediately after graduate school, as a newly minted Ph.D., I headed the physical oceanography program aboard the *Eltanin*, which was in the midst of a ten-year quest under Lamont's direction to survey the entire southern ocean. At that time, one was placed directly into the fire, with no post-doc buffer to ease one into the realities of running a research program.

In many ways, ship-based oceanography in the 1960s wasn't very much different than it was in the 1920s aboard Wüst's *Meteor*. On the *Eltanin*, we remained at sea for sixty days, spending half that time getting to and from the research area. We collected water in Nansen bottles, taking samples at only thirty stations, measuring at only twenty-three different depths. We struggled to collect those few samples.

Today we can sample continuously from sea surface to seafloor at 100 stations in a thirty-day cruise. Satellites now provide images of the sea surface temperatures over the entire ocean. They monitor sea-level changes and wind stress on the ocean surface and look at sea ice cover hiding under the clouds. When I now look at ocean intricacies that are exposed so relatively easily by satellite images, I wonder at the value of the heroic efforts we put into getting those very few hard-earned data points someplace out in the middle of the complex ocean. But we had lots of time to think about each data point. We extracted every last bit of information from those points and valued all of them.

Had we known how complicated the oceans are, might we have been too disheartened to move ahead? I don't think so: The excitement of discovery outweighed any sense of discouragement.

It is the feeling of exploration, of adventure, that creative act of building a story from an imperfect set of

observations that fueled, and continues to fuel, my love of oceanography. The story must be consistent with the physics of fluids, but it is completed with the imagination.

The unfolding story is revealing just how complex and dynamic an environment the ocean is. Tides and currents are constantly moving water around the globe. Some currents are driven along the sea surface by the winds, shaped and steered by continents in their way. In some parts of the ocean, the atmosphere makes surface waters become colder or saltier, and therefore denser, causing them to sink to the abyss. In other parts, waters are warmed or less salty, and therefore more buoyant, causing them to rise. These factors drive currents downward and upward within the ocean, and together with wind-driven currents, form the ocean's circulation.

The properties and processes that circulate water around and through the world ocean are the engines that drive Earth's climate. For the oceans are also vast reservoirs of heat and energy, and as they move, they transfer that energy around the planet and prompt exchanges between the ocean and atmosphere. Warm surface waters flowing northward from the tropics give up their heat to the atmosphere in the North Atlantic, and that's what keeps Scandinavia warmer than Greenland in the winter. The periodic shift in tropical Pacific waters spawns El Niño every few years. On longer time scales, changes in global ocean circulation probably triggered abrupt, large-scale climate shifts in the past and, according to many scientists, may do so again in the future.

Only over the past few decades have we discovered the interconnections between the oceans and atmosphere, and between many regions of the ocean. Earth's climate is a multifaceted system, and the challenges we face today on climate change, air and water pollution, fisheries and other environmental problems demand that we understand the complex interactions that make the whole Earth system go.

Throughout my career, I have sought to unravel these interwoven processes. Steeped in the Lamont tradition, I have been particularly attracted to ocean regions that have not received much previous scrutiny, places that I consider have a key role in the global scheme of the ocean, where I can



take that first exploratory and discovery step. I have explored the Southern Ocean, the South Atlantic, and, more recently, the Indonesian Seas and Indian Ocean. Though technologies have continually improved, the strategy has remained essentially the same: to take measurements of temperature, salinity, dissolved gases and nutrients (and, more recently, subtle chemical tracers) to track the direction and speed of water's movement and mixing within the oceans.

My South Atlantic work has dealt with a fundamental question of oceanography: Why is the North Atlantic so salty? The answer to that question holds a key to understanding Earth's entire climate. Because the upper layer of the North Atlantic is salty, it is made denser as it is cooled in its polar reaches, and sinks into the deep ocean—pushing water throughout the world's oceans like a great plunger. The phenomenon sets in motion a sequence of globally interconnected ocean currents, which govern our climate by transporting heat and moisture around the planet.

Today, for example, the process that starts with sinking in the North Atlantic comes full circle by eventually propelling warm surface waters, including the Gulf Stream, back into the North Atlantic. Therein lies the answer to my college-days question about the climate difference between Greenland and Scandinavia. In winter, the warm surface waters transfer their heat to frigid overlying air masses that come off ice-covered Canada, Greenland and Iceland. Thus tempered, the eastward-moving air masses make northern Europe noticeably warmer in winter than comparable latitudes in North America.

But the system is delicately balanced. Changes in its operation and vigor have been linked to important variations in Earth's climate in the past, and its susceptibility in the future may be a crucial factor in determining the planet's potential for abrupt climate change.

According to the conventional wisdom, the North Atlantic is saltier because more evaporation occurs there, leaving excess salt in the ocean. In addition, because of prevailing westward winds in Earth's tropical regions, fresh water from the Atlantic (in the form of water vapor) is exported across central America to the Pacific (at a rate of about 0.3 million cubic meters per second).

In contrast, I think the excess evaporation in the Atlantic is not the cause, but rather an effect that reinforces the whole cycle. In my view, the very sinking of North Atlantic waters draws warm surface water northward to fill the void. That brings warm waters in direct contact with a colder atmosphere, which increases the evaporation of warm water to the atmosphere, leaving the surface colder and saltier to continue the process.

In November 1983 I found what I believe is the more fundamental reason for a salty Atlantic. I was aboard Woods Hole Oceanographic Institution's research vessel *Knorr* just off Cape Town when I found a patch of water that wasn't quite what I expected to find in the South Atlantic. It was too warm and salty. After much thought I realized it was an intruding large pool or eddy from the Indian Ocean. The Agulhas Current rounds the southern rim of Africa only to be turned back into the Indian Ocean in what is called a retroflexion. But I found that this retroflexion is not complete: The Agulhas sheds large eddies and filaments of warm salty Indian Ocean water into the South Atlantic. (The equatorial Indian Ocean is heated by the tropical sun, which spurs evaporation that leaves that ocean relatively salty.) The Agulhas Retroflexion boosts the salt of the South Atlantic, which eventually is drawn northward into the North Atlantic.

I also found another contributing factor: Investigating the Antarctic Circumpolar Current, which flows around the continent at the bottom of the world, I discovered that the current becomes less salty as it passes from the Drake Passage across the South Atlantic sector toward the Indian Ocean. This results from an infusion of fresh water dumped into the circumpolar band by rainwater drawn from evaporation over the South Atlantic subtropical region. Flowing eastward, the powerful Antarctic Circumpolar Current removes this fresh water from the South Atlantic and passes it into the Indian and Pacific Oceans. As a result of these combined processes, the Atlantic gets saltier, and the Indian and Pacific Oceans get fresher.

When I expressed this view in a controversial 1986 research paper and a follow-up publication in 1992, it

spawned a plethora of studies by other scientists who made their own observations or used computers to model global ocean circulation. Some say I'm wrong; others say I'm right. The story is not over, but my research has provoked people to reassess their thinking about ocean circulation and look at it in a different, more global way.

Similarly, another set of unexpected observations in the Southern Ocean prompted me to come up with a previously unconsidered hypothesis to explain another oceanographic mystery: the Weddell Polynya—a large swath of open water in a sea that is usually uniformly covered by sea ice. During the austral winters of 1974, 1975 and 1976, near the Greenwich Meridian and 66°S, the advent of passive microwave data from satellites revealed that a large region of the Weddell Sea had not frozen over. With no previous satellite data, it was tempting at the time to consider the Weddell Polynya as the norm, but a large, persistent Weddell Polynya has not occurred since 1976. Apparently something happened to change the way the ocean works in the Weddell Sea in the mid-1970s.

In February 1977, while in the center of the Weddell Sea, I was most surprised to notice that the normal layering of that sea was gone. What I expected was what was observed in the past: a cold surface layer about 100 meters thick lying atop a warmer, thicker layer of water that originated from more northerly latitudes. Instead I found that the water was nearly uniformly cold from the surface to 3,000 meters down. What happened? Was the instrumentation broken? I checked, but that wasn't it. I suddenly realized that this might be the explanation of the Weddell Polynya. Something happened to cause the ocean to actively overturn, breaking down the previous two-tiered layering of the ocean, bringing deep-water heat to the sea surface and deterring the formation of sea ice. The sea had abruptly changed from one stable mode of operation to another, from a stratified ocean to a convective ocean.

The Weddell Polynya became a hot research topic and remains so, as scientists try to prove its cause. Even more important, we are trying to assess the chances that it may occur again or that the Weddell Sea may lapse into a long-lasting polynya-forming mode of operation in the future—

which could have an impact on global ocean circulation and Earth's climate.

Fifteen years later, I was back in the Weddell, but farther west, in a part of the world's ocean that even in the last decade of the twentieth century remained essentially unexplored because it remains covered by ice throughout the year. Only one other expedition had ever entered this forbidding area. In 1915, Ernest Shackleton's aptly named ship *Endurance* became trapped in the ice and over more than nine months drifted 570 miles north, where it was finally crushed by the ice pack and sank. Escaping onto an ice floe, Shackleton's crew dragged boats to open water and reached Elephant Island, from which Shackleton and five others crossed 870 miles of ocean and organized the rescue of every man.

The perennial sea ice, and Shackleton's experience, perhaps, had deterred subsequent exploration of the region. Yet from observations obtained outside the region, we suspected that the Western Weddell has an important role in the Earth's global ocean circulation and climate. In the Western Weddell, very cold, dense water cascades off the continental shelves east of the Antarctic Peninsula. These waters sink and spread along the seafloor, feeding and cooling most of the world's ocean depths. In addition, the Weddell's sea ice acts as an important border crossing between the ocean and atmosphere and the critical exchanges of heat and greenhouse gases that help regulate the climate. The stability of Weddell sea ice in the face of human-induced climate changes, such as global warming caused by fossil fuel burning, is a critical, unresolved question.

To explore this frontier, we borrowed a successful method Lamont had long experience with in the Arctic Ocean: deploying a scientific station on a drifting ice floe. The Russians had even more experience doing that, so we proceeded in another Lamont tradition: conceiving and implementing large-scale, international, multidisciplinary, multi-institutional expeditions in the pursuit of basic science.

In 1988, in the waning days of the Soviet Union, I met with colleagues from the Arctic and Antarctic Research Institute in what was then Leningrad to begin organizing



Ice Station Weddell. In February 1992, thirty-two American and Russian scientists and support personnel set up camp on a 6.5-foot-thick, 1.7-square-mile ice floe at 71°S and 50°W. Generally following the path of the *Endurance*, Ice Station Weddell drifted 465 nautical miles north over four months. In the spirit of basic exploration of an unknown region, the science program spanned many disciplines, studying sea ice, ocean currents, meteorological conditions and marine biology. It provided the first hard data and observations of this important part of the global ocean, which scientists can now plug into their numerical models that seek to simulate Earth's ocean and climate systems.

Sophisticated computer modeling has become a major tool for investigating and understanding the complex dynamics of the ocean and the climate, but the foundation, and the validity, of any model still relies on actual observations. That is as true for the frigid Weddell as it is for the tropical Indonesian Seas, where my research has taken me most recently. Colleagues have joked that I finally learned, late in my career, to do research in more congenial climes. But in truth, geopolitics had prevented exploration of this region, leaving it one of the last oceanographic frontiers.

The archipelago of Indonesian islands presents a critical bottleneck in what would otherwise be an unimpeded highway of tropical ocean stretching more than 16,000 miles from South America to Africa. Indonesia creates a narrow, complex passageway, inhibiting free flow between the Pacific and Indian Oceans and presumably causing a vast pool of warm waters to accumulate in the western tropical Pacific. The periodic movement of that warm pool, and the accompanying rain-filled low-pressure system that forms above it, are the essential ingredients of El Niño and the Southern Oscillation, the phenomenon that has far-reaching impacts on short-term global climate.

The exchange of warm ocean waters between the Pacific and Indian Oceans, or what is often called the Indonesian throughflow, is influenced by and may influence ENSO and perhaps the timing and strength of the Asian monsoons, whose moisture comes from water evaporating

from the Indian Ocean. Complicating the picture even further is the fact that the Indonesian Seas have some of the world's strongest tides, which promote mixing of relatively fresh warm surface waters with deep waters that are colder, saltier and nutrient-rich. This strong vertical mixing, among other things, affects the properties of the waters entering the Indian Ocean and also causes marine life to thrive.

In the early 1990s, after years of difficult negotiations, I developed with the Indonesians the first expeditions to collect basic data on how water flows through the maze of Indonesian islands—how much, how warm, how fresh, how fast, which routes, etc. To date, I have participated in five expeditions aboard Indonesian research vessels.

In 1995, I extended my tropical work, attempting to trace the pathway of the Indonesian throughflow into the Indian Ocean. The Pacific adds relatively fresh water to the Indian Ocean, where evaporation leaves behind a wealth of salt. Part of this water may pass around the southern rim of Africa, contributing salt to the Gulf Stream into the North Atlantic. Hence my Indonesian research has brought me full circle, linking to my earlier South Atlantic research and the global theory of ocean circulation that I first promulgated in 1986.

Nearly four decades after I first came to Lamont, I'm using ship-based and satellite-based technology that Wüst could only imagine and linking my observational research with global and regional computer models of the oceans. And I've never lost the feeling that unexpected "discoveries" are still lurking out there. The division between known and unknown in science is not a sharp boundary between light and dark. It's more like that "murky mist" that frustrated Ewing. Scientists don't push back the darkness in a uniform advance. Instead, someone with an idea makes a leap, creating a small circle of light amid the darkness. Others then focus on that isolated patch, and even if they disprove the new idea, knowledge still advances. Sometimes, though, those risk takers are right.

Lamont always challenged its researchers to take that jump deep into the unknown.



*Among the questions the expedition  
proposed answering were:  
“What kind of climatic changes were there in  
prehistoric and historic times?  
What was the impact of the climatic fluctuations  
on the evolution of Man?”*





# THE ANSWER WAS BLOWIN' IN THE WIND

## *Deep-Sea Drilling and the Link Between Climate Change and Human Evolution*

by Peter B. deMenocal

I had the “two o’clock meanies” and it wasn’t even noon yet. The high African sun beats on the brow in a relentless, almost physical way and you can feel the dry heat creeping into your skull no matter what is covering it. There’s no vegetation and therefore no shade in the desert, so as the sun builds toward its peak you tend to stumble about in the hope that the next dip or ridge might be less hot or have a wisp of breeze. But you soon discover that everywhere is equally hot and it just gets hotter until about two o’clock, when temperatures taper off to merely torrid levels. Just after noon, the sun’s downward piercing glare meets the wavy heat that has built up through the morning from the radiant desert floor. The heat can become so intense that you become lightheaded and irritable. At its worst, a mild dementia sets in. These are the two o’clock meanies, and in small clans of similarly afflicted people it makes for interesting times.

We were near Lake Turkana in northern Kenya in the summer of 1993, studying sediments that once lay on the lake bottom but that were now exposed on the margins as the lake had receded over time. The sediments held clues to the climate history of the region. Our scientific objective was to find out how lake levels rose and fell in the past. With that knowledge, we could gauge when the region’s climate was wetter and drier over the time span when our first ancestors—then fully bipedal, rather short in stature and just about to discover tool-making—were roaming these same shores.

My colleague on this, my first field trip to Kenya, was Frank Brown, a geologist from the University of Utah. He had

decades of experience working with anthropologists Richard and Meave Leakey and studying these East African sediments and the early hominid fossils they contain. Brown had just picked me up from the airport in Nairobi, having driven three grueling days from Turkana only to collect me, turn around, and make the same journey back. It was he who educated me on the finer points of the two o’clock meanies after watching me over the first few days stumbling around the desert, dizzy with heat.

As often happens in science, Brown and I had met by chance encounter. In 1993 Elisabeth Vrba, a paleontologist at Yale University, organized a meeting that brought together scientists in various areas of expertise to discuss research on the history and evolution of African hominids and other vertebrates—as well as the parallel history of African climate and vegetation changes—over roughly the interval spanning the known fossil record of our earliest human ancestors in Africa.

I had heard that paleoanthropology was a fiery scientific community and that the topic of climate change and human evolution was particularly contentious. That made me a bit nervous because I was about to give a talk that presented a new perspective on the subject. Our new contribution to this age-old enigma was this: For the first time, we analyzed sediments drilled from the bottom of the ocean, gleaned physical evidence—which was much more complete, detailed and well-dated than any that could be found on land—of how the African climate changed over the past several million years. During that same time interval, huge changes occurred in the faunal and floral



landscape of subtropical Africa—shifts that ultimately led to the emergence of our own species.

Scientists as far back as Darwin in his *Origin of Species* have proposed links between African climate and evolution, but there has always been a dearth of paleoclimatic evidence to support this view convincingly. Long-term changes in African climate from more humid, lush conditions to more arid, open conditions have long been cited as a key factor underlying the broad evolutionary trend from more apelike to more human characteristics, including bipedality, increased body size, tool-making and vastly larger brains. The so-called “Savanna Hypothesis” essentially states that past shifts to more open grassland conditions created specific ecological, physical and behavioral challenges that selectively favored the emergence of our larger, bigger-brained ancestors.

To be sure, abundant evidence exists that Africa was much wetter in the past, but the evidence preserved in rock layers on land is commonly incomplete. On land, erosion and the geological forces that create earthquakes and build mountains both act to erase the past. As Brown and I explored the desert terrain of northern Kenya, a region that today receives effectively no rainfall, we occasionally encountered stark reminders of wetter past climes in the form of enormous petrified tree stumps and trunks poking through the rust-colored desert sands. These were remnants of an ancient tropical forest that now appeared glassy and wind-frosted and distinctly out of place under the desert sun. We found thick strata of a pure white rock called diatomite, composed entirely of the glass-like skeletons of microscopic lake algae. These strata also contained enormous fully articulated skeletons of ancient lake fishes, some reaching two meters in length. These were powerful indications of past wetter African conditions, but these records are so few and far between and so poorly dated that they provide little more than circumstantial evidence in documenting any past causal relationship between the changes in African climate and in African fauna.

Our strategy for reconstructing long, detailed records of past changes in African climate took advantage of the unique evidence preserved in deep-sea sediments. These sediments

accumulate slowly but continuously over millions of years. They can be extremely well-dated using a variety of methods and they remain relatively undisturbed at the ocean floor.

In the late spring of 1987, I was aboard the *JOIDES Resolution* (Joint Oceanographic Institutions for Deep Earth Sampling), the research vessel of the international scientific Ocean Drilling Program (ODP). At 140 meters long, the *Resolution* is a large ship—made even more impressive by a drilling derrick looming sixty-five meters high in the ship's center. The *Resolution* also has five decks housing state-of-the-art laboratories for conducting real-time scientific investigations on the sediments and rocks recovered by deep-sea drilling. Any of these labs would be the envy of a university geoscience department, being fully equipped with superb research equipment and staffed by highly trained, full-time technicians.

On this cruise, Leg 117 of the Ocean Drilling Program, the *Resolution* was scheduled to drill off the coast of Oman in the Arabian Sea on a two-month voyage in the northwest Indian Ocean from Columbo, Sri Lanka, to Port St. Louis on the island country of Mauritius. I was a graduate student at Lamont at the time, wide-eyed in awe of the ship's size, its large, gleaming laboratories and, of course, the exotic ports of call. The cruise had been proposed and planned by Warren Prell, a Lamont graduate and then a paleoclimatologist at Brown University. Prell and other colleagues from Brown had studied these sediments for many years and had identified several key clues, in fossils in the sediments and in the sediments themselves, that could shed light on past variations in the strength of the Indian monsoon. Prell and colleagues had proposed that the sediments we were about to drill would give us the first detailed evidence of when the Indian monsoon first started many millions of years ago, as well as other information about related changes in African climate.

The word “monsoon” often conjures up images of tropical climes, with sweltering heat and sudden, torrential rains that flood the landscape. The climatic phenomenon we call the monsoon derives from the Arabic word *mausim*, which roughly translates to the word “season.” Monsoonal regions

are very seasonal, with hot, wet summers contrasting with very dry, rainless winters. This monsoonal climate occurs over a huge swath of land spanning from west Africa, across Arabia, and throughout southern and southeastern Asia. The strong seasonality of rainfall and winds in monsoonal regions results from the simple fact that the sun can heat a slab of continent far more easily than it can a comparably sized area of ocean. During summer months, when the sun reaches its highest point in the sky, the land heats up faster and more efficiently than the adjacent ocean. This establishes a huge, continent-scale "sea breeze," as warm air rises over the land and draws in moist, maritime air from adjacent oceans. The opposite happens during the winter months: The land cools more efficiently than the oceans and a "reverse sea breeze" blows dry continental air out toward the oceans. This seasonal reversal in winds and rainfall is the essence of monsoonal climate. The strength and expanse of the African and Asian monsoons result from two facts: These continents stretch across equatorial latitudes where solar heating is greatest, and they are surrounded by large oceans that feed water to the atmosphere.

What could we measure in deep-sea sediments that could be used to deduce past changes in the monsoons and in African climate in general? Africa's monsoonal climate gives it relatively wet summer seasons in which ephemeral lakes fill and savanna grasses and shrubs sprout from the barren land to create a sea of green. But the ensuing winter, with its strong, dry winds, desiccates the landscape and dust storms blow surface soils and their mineral particles, collectively called "dust," out to sea, where they eventually settle in the ocean to become deep-sea sediments. Nearly half a billion tons of African dust are transported by the winds to the oceans each year. Charles Darwin commented on these African dust storms during his 1846 *Beagle* voyage off Cape Verde: "... During our stay of three weeks at St. Jago [Cape Verde], the wind was N.E. as is always the case this time of year; the atmosphere was often hazy, and very fine dust was almost constantly falling, so that the astronomical instruments were roughened and a little injured."

Modern studies have shown that year-to-year variations in the amount of this African dust in the atmosphere over the Atlantic Ocean is very closely tied to the amount of rain that falls in the Sahel and sub-Saharan regions of Africa—the source areas for the dust. Not surprisingly, when there is less rainfall, it is drier and the winds pick up and transport more dust to the ocean. This same linkage between regional aridity and atmospheric dust concentrations is also found off northeast Africa and Arabia, with dusts being carried by winds to the northeast Indian Ocean.

Using this same relationship, we hoped to deduce past changes in African climate by carefully measuring wind-blown dust concentrations preserved in ocean sediments from several locations off East and West Africa—records that would be largely impossible to find on the African continent itself.

These wonderful archives of past climate changes ordinarily would have been inaccessible to us, since they lay two to five kilometers below the surface of the ocean. Our tool to recover these all-important records was the Ocean Drilling Program, one of the world's largest, longest-lived and most productive international scientific joint ventures. Not surprisingly, Lamont scientists play a major role in ODP today—using drilled cores for their own research and creating and using drilling tools that they and other scientists use on expeditions. Nor should it come as a surprise that Lamont played a central role in launching a global deep-sea drilling program.

Ocean drilling was first proposed as a research tool in the mid-1950s by Walter Munk of Scripps Institution of Oceanography and Harry Hess of Princeton University, who became excited about the possibility of pushing the limits of offshore drilling technologies to drill deeply—very deeply—through the ocean crust all the way to the boundary with Earth's upper mantle. This boundary, known as the Mohorovicic discontinuity, or Moho, lies an impossible thirty to fifty kilometers below continents, but only five kilometers below the seafloor. It was a tantalizing but still ambitious target, a feat made much harder by adding the difficulties of drilling in water many kilometers deep.



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The operational and technical scale of what they were proposing was enormous.

The idea took root Munk's home in La Jolla, California, at a breakfast meeting of the unconventional American Miscellaneous Society (AMSOC), a loose affiliation of earth scientists who met, drank, laughed and usually took the lighter side of heavier problems. But the vision of ocean drilling was so alluring that AMSOC got serious, coordinated its efforts and submitted a feasibility proposal to the National Science Foundation (NSF). The proposal initially was turned down because of the lack of a formal organizational structure to guide such an ambitious effort. By sheer tenacity, AMSOC members subsequently reformed themselves into a National Research Council committee, reconsidered their strategy, and resubmitted a formal proposal in 1957 to drill a single hole deep into the ocean's crust to penetrate and sample the Moho.

By this time, Maurice Ewing, Lamont's director, was already in on the action, having become a member of AMSOC. But from the start, he cautioned against committing all possible resources to a single, very expensive and risky deep hole. Instead he urged that the scientific community first gain some experience in drilling and recovering samples in great water depths by attempting many shallower holes, perhaps a kilometer deep, through softer ocean sediments and some upper underlying oceanic rocks. No doubt, Ewing had some ulterior motives, both practical and scientific. He believed in a strategy of assembling a comprehensive picture of the oceans by accumulating data from many places—as evidenced by his unremitting pursuit of data, including short sediment cores, collected daily by Lamont's research ship, the *Vema*. More pragmatically, Ewing also knew that the best sites for drilling would most likely be identified by seismic and other geophysical surveys—performed by the *Vema*. The great debate—to drill many shallower holes or a single deep hole—divided the AMSOC group as well as the earth sciences community, but in the end, the group decided to move forward with a proposal to charter a ship to drill a five-kilometer hole into ocean crust off the Mexican island of Guadalupe in five kilometers of water. The project, which came to be known

as the Mohole Project, was funded in 1958 with an initial \$2.75 million budget.

To a large degree, the decision was deeply rooted in the times. As Kenneth Hsü put it in his book, *Challenger at Sea: A Ship that Revolutionized Earth*: “In 1957, two years before the Americans decided to send a man to the moon, nothing seemed to cost too much, and nothing seemed impossible.” That year also marked the successful launch of the Soviet Satellite Sputnik. Deep drilling became fueled by Cold War machismo when the U.S.S.R. again sought to assert its technological superiority over the U.S. by proposing its own deep-drilling program. In some ways, reaching the Mohole turned into a goal in itself, not unlike landing a man on the moon.

The group chartered an industry vessel, the *CUSS-1*, and in 1961, after several trials in soft sediments, a 183-meter hole was drilled into the ocean crust. It was far short of the target depth, but it demonstrated the feasibility of scientific ocean drilling. In particular, the test proved the viability of a new technology: a “dynamic-positioning system” that kept the ship from drifting in open waters. In the system, sound-emitting beacons anchored to the seafloor sent acoustic signals to computers on the ship, which activated thrusters that constantly repositioned the ship above the drill site.

By now, the Mohole Project had taken on sharply political dimensions. A Texas congressman became chair of the NSF budgeting committee, and a contract to build the deep-drilling vessel was awarded not to the lowest bidder or to the most experienced contractor, but rather to a firm that happened to be located in Texas. It probably didn't hurt that the vice president at the time also happened to be a Texan, nor that Texas was the heart of oil-drilling country. The decision was subsequently challenged and overturned, causing further expense and delay. Amid the politics, the Mohole Project soon mushroomed into a much more ambitious effort, and a total of \$57 million would be pumped into a fiasco of bidding and rebidding and cost overruns to extend the drilling technology. The price tag was still pennies compared to the billions of dollars that were going into the space program, but nevertheless, the apparent inefficiency of the Mohole Project (along with the



sudden death of the Texas congressman in 1966) led to its demise. In the end, there was no ship nor Mohole to show for the money; the project's best achievement had been a 300-meter hole drilled on land to test drilling tools. Congress killed funding for the project and the idea of scientific ocean drilling faded.

That could well have been the end of it, but the persistence of a few key scientists, including Ewing, managed to resurrect it by launching a concurrent initiative that moved away from the go-for-broke Mohole strategy. Ewing had never abandoned the idea of drilling many shallower cores, and he found an equally determined ally in Cesare Emiliani of the University of Miami. Emiliani had pioneered the use of isotopic measurements, as well as measurements of fossil shells, in short piston cores to reveal the waxing and waning of continental glaciers in Earth's history. He argued persuasively that there was great promise in using deep-ocean drilling to obtain longer cores to extend these records into the more distant past. In 1962, he proposed a modest program to drill many locations in the Caribbean Sea to recover sedimentary sequences to decipher Earth's glacial history over the past several million years. The NSF was interested, but it soon became clear that Emiliani's proposal was beyond the resources of a single oceanographic institution.

Ewing jumped aboard, lending his considerable weight in an active campaign for such a drilling program. A committee of scientists called LOCO (for LOng COres) was formed to design and coordinate the drilling program, and the proposal gained momentum. Ewing was instrumental in establishing a new organization that subsumed LOCO. Founded by Lamont, Miami, Scripps and Woods Hole Oceanographic Institution, it was the first consortium of oceanographic institutions working together on large-scale scientific efforts. In 1965, as Project Mohole was in its death throes, the JOIDES emerged. It was no small feat to forge unity across the community. In his memoirs, Joe Worzel, Lamont's longtime assistant director, facetiously said the name JOIDES (pronounced "joy-dees") was picked because of all the "joyful" time spent in organizing committees.

Lamont was given the responsibility of managing the first JOIDES drilling leg. In a typical Lamont piggybacking maneuver, it seized an offer from an oil company to borrow its chartered ship, the *D/V Cadrill*, while it was en route from Panama to Canada—thus saving the cost of getting a ship to and from the drill site. Six holes were drilled across the Blake Plateau off the Atlantic coast of Florida in water depths of only twenty-five to 100 meters. The site was ideal for using recovered sediments to reconstruct the positions of the shoreline over time, and thereby to measure sea-level variations in the past. The leg was highly successful and, with the demise of the Mohole Project, the scientific community and Congress quickly coalesced behind the commissioning of a larger, more sophisticated drill ship dedicated to JOIDES.

Scripps was awarded the initial eighteen-month contract for the Deep-Sea Drilling Program (DSDP), but the entire consortium, JOIDES, was responsible for DSDP's scientific objectives, drill sites, shipboard measurements and scientific staffing. Congress allocated \$13 million to acquire and refit a drilling vessel that could drill the ocean's depths for months at a time in moderate seas and hold station using dynamic positioning. Scripps subcontracted with Global Marine Inc. to provide the drilling vessel, christened the *Glomar Challenger* after the legendary eighteenth century oceanographic research vessel *HMS Challenger*. It was ready in 1968, and Doc Ewing and Joe Worzel were selected as co-chief scientists of the first DSDP leg to explore the Sigsbee Knolls, a group of mounds that protruded hundreds of fathoms above the adjacent seafloor in the Gulf of Mexico, some 640 kilometers southwest of New Orleans.

Ewing and Worzel had surveyed the area with the *Vema* and were persuaded that the knolls were salt domes—created when less dense salt, naturally seeking to rise, pushed upward on overlying sediments that buried it. At that time, most people did not believe such geological features could form in deep water. The debate was resolved immediately by the presence of salt in the drilled cores. And the cores also contained another surprise: oil. It was the first direct evidence of oil in ocean depths deeper than continental



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shelves, a harbinger of a new petroleum province that is now being harvested today. The drilled knoll subsequently was named *Challenger Knoll*, in honor of the ship.

Ewing and Worzel knew exactly where to drill on that first leg because of geophysical data they had collected aboard the *Vema*. In many ways, the initial success of DSDP was largely due to the pre-existing wealth of bathymetry, seismic profiling, and magnetic and core data that Lamont had collected and archived. Those data allowed scientists to determine very precisely the best drill sites to test an idea or hypothesis.

The advent and application of global positioning technologies also contributed mightily. In the early 1960s, Worzel had talked an admiral in charge of the Navy's Special Projects branch into letting Lamont "borrow" one the Navy's six experimental military satellite navigation (SATNAV) systems. Worzel proposed that a SATNAV system be installed on a Lamont ship during a month-long Christmas hiatus while Navy ships were idle. Lamont graciously would put the new system through a series of tests to determine its accuracy for the Navy, he said. With a knowing smile, the admiral agreed. A month later, when Worzel bargained to keep the SATNAV system for a little longer, the admiral smiled again and said that he hadn't really expected to get it back. After a year of using SATNAV, Worzel returned to Special Projects, proposing that the Navy give Lamont its classified plans for the SATNAV system, so that Lamont could build its own and return the Navy's! Four satellite systems were built in Lamont's electronics shop, one each for the *Conrad* and *Vema*, one for Ken Hunkins' ice floe camps and one spare. Lamont's ships were the first in the academic research fleet with the capability to fix exact positions in otherwise featureless oceans. SATNAV was somewhat limited by the relatively few number of satellites flying in those days (sometimes crews had to wait ninety minutes between satellite fixes), but it gave scientists an unprecedented ability to find and return to specific locations to resample, remeasure or drill deep-ocean features.

On one early DSDP leg, like an advance scout for the oncoming cavalry, the *Vema* gathered data in the Caribbean to help locate the best drill site. Scientists and crew aboard

the *Vema* collected bathymetric, magnetic and gravity data and worked them up through the night. When the *Challenger* showed up the next day, Lamont scientists went over in a small boat, handed over the data charts and pointed to an appropriate site for drilling.

Deep-sea drilling made the other three-quarters of the world available for sampling and exploration. The slender cores penetrated deep into the seafloor and into millions of years of Earth's geological history, revealing clues about the planet's origin, climatic evolution and present-day structure. Scientists from a variety of disciplines examined the cores to learn about the rearrangement of continents, the evolution of life in the sea (and on land), and the history of changes in global climate, ice sheets, ocean currents, worldwide sea levels and the Earth's magnetic field.

One of the early highlights of the drilling program occurred in 1970 on Leg 13, whose co-chief scientists were Lamont's Bill Ryan and Kenneth Hsü of the Swiss Federal Institute of Technology of Zurich. Drilling in the Mediterranean Sea, the scientists found materials commonly found in shallow lagoons or desiccated seabeds. They made the startling discovery that the Mediterranean had once been isolated from the Atlantic, and its waters had evaporated to create a desert where now there is a sea.

Many technological lessons were also learned in these early days. New coring procedures were instituted to recover cores that were as continuous and as complete as possible, to ensure that there would be few "missing pages" in the record of Earth history. New coring technologies were developed over time to ensure that the sediment records came up undisturbed, so that the "pages" would not be torn or mangled. Core recovery and quality were also improved by new technology to dampen the stresses, caused by the ship bobbing up and down, on the drilling equipment dangling beneath the ship. A "can-do" attitude among scientists, engineers and operators alike drove DSDP forward.

The program became so successful and its capabilities so unique that other countries sought formal agreements to secure regular scientific access to the ship, and the

International Phase of Ocean Drilling was established in 1975. The partner countries at the time (France, England, West Germany, the U.S.S.R. and Japan) each paid a share of the program's operating costs and contributed national resources to plan and prepare for proposed drilling legs. The internationalization of the project broadened its scope and exposure considerably, making DSDP one of the most successful and longest-lived international scientific joint ventures. DSDP completed ninety-six legs between 1968 and 1983. Over that time the *Glomar Challenger* logged more than 600,000 kilometers and collected nearly 100 kilometers of core, which are archived at Lamont, Scripps and Texas A&M University. The scientific results far exceeded expectations, and some of the most notable discoveries can best be described as revolutionary.

In 1983, a downturn in the oil industry presented an opportunity to get an affordable long-term lease on the dynamic positioning BP/Schlumberger drill ship BP471. To honor Cook's eighteenth-century exploring vessel and to convey its renewed determination, the ocean drilling community rechristened the ship *JOIDES Resolution*. With a long-term commitment from NSF for \$22 million, the new ship was completely refitted with modern analytical laboratories, core scanning and archival facilities, and advanced drilling and coring technologies, and it gave birth to a new program, the Ocean Drilling Program (ODP), whose scientific operations were managed by Texas A&M University.

As an addition to ODP operations, Roger Anderson at Lamont established the Borehole Research Group, which takes advantage of the holes drilled by the *Resolution*, using them as entry points to send an array of sensors deep into the Earth. Into the seafloor holes, Lamont researchers use a wireline to suspend electronic instruments that measure temperature, conductivity, magnetism, porosity and other characteristics of the rock lining the hole. The measurements are recorded continuously as the instruments are drawn back up the hole, and they complement the information scientists glean from analyzing the actual rocks and sediments brought up in the core. In the Lamont tradition, borehole measurements have been archived and made available to the scientific community.

Altogether, ODP was a quantum leap beyond the now-dated DSDP technologies. The facilities were better, the drilling and stationing technologies were modernized, and the research program was invigorated with new ideas from the scientific community worldwide. ODP was designed from the start as a fully international research effort, with partner countries (Canada, Australia, France, Germany, Japan, the United Kingdom, and the European Science Foundation, representing Sweden, Finland, Norway, Iceland, Denmark, Belgium, the Netherlands, Spain, Switzerland, Italy, Greece and Turkey) collectively contributing somewhat less than half of the operations budget, with the remainder covered by NSF.

ODP drilling and research have pushed our knowledge of Earth history to a much higher level. They have advanced our knowledge of how and why the world has plunged into and out of glaciations; how the ocean's deep circulation and vast carbon dioxide reservoir are linked to abrupt changes in climate; how global sea levels rose and fell in the past; how fluid pressures affect faulting and earthquakes; how the oceans and continents have evolved; how materials and chemicals are recycled in subduction zones, where old oceanic crust plunges back down to the mantle, and at mid-ocean ridges, where new seafloor is created from mantle material. ODP legs have explored a potentially vast new source of energy, gas hydrates—ice-like deposits of crystallized methane and water that form under higher pressures and frigid temperatures in the deep sea. They have found evidence of a massive previously unknown microbial biosphere living in oceanic rocks. Just a few years ago, an ODP leg achieved the near-perfect recovery of the meteorite glass remnants of the extraterrestrial asteroid impact that snuffed out the dinosaurs (and much of marine life as well) about 66 million years ago at the end of the Cretaceous Period.

One of the most enduring scientific dividends of the program will be the establishment of a very accurate chronology of the entire 66-million-year record of the Cenozoic Era, an ongoing decade-long project led by Sir Nicholas Shackleton of Cambridge University. Using sediment records from various ocean basins, he and other colleagues are reconciling all



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available dating methods (including telltale layers of fossil shells, and magnetic and isotopic data). The resulting master time scale will provide a precise calendar on which to lay events in Earth's geological history.

As of 1999, more than 1,200 scientists from twenty-five countries had participated on sixty-five separate, usually two-month expeditions aboard the *Resolution*. Each leg is staffed by some of the world's experts in geochemistry, geophysics and paleontology, making the *Resolution* a seagoing geoscience research institute. The ship has now circumnavigated the globe, logged a half-million kilometers, and drilled in every ocean basin, from the polar Greenland Sea to the sweltering Arabian Sea, where she parked in 1987, with me aboard.

It was late summer and balmy sea breezes were wafting on the deck when the ship reached our drilling locale off the coast of Oman. The crew first released a chirping beacon over the side, which rapidly sank to the ocean depths and tagged the position. Then the crew hydraulically lowered twelve large thrusters from concealed pods within the ship's hull. A set of acoustic transceivers mounted on the hull listened to the chirps coming from the seafloor. A central computer used this acoustic information to rotate the thrusters and dynamically position the ship directly over the beacon location.

The ship's thrusters were churning to position the ship over a precise location on Owen's Ridge, a geologic scar on the ocean floor extending from Somalia northeast to Pakistan. Our drill site was on a plateau-like feature on the crest of the Owen's Ridge, which rose 2,000 meters above the average depth (4,000 meters) in this part of the northeast Indian Ocean. We had selected this site because previous work on Lamont sediment cores had shown that the sediments here, a mixture of microscopic fossils and wind-blown dusts and clay from Africa and Arabia, had accumulated very evenly and regularly over the past millennia without disturbance by slumping or erosion.

Once on location, the ship's crew scrambled to begin drilling. This is an impressive operation. Huge thirty-meter stands of high-tensile steel drill pipe are raised up the drilling derrick, coupled together, and lowered to reach the seafloor

(to a maximum depth of eight kilometers). The stands of pipe are screwed together using an "iron roughneck," which resembles an enormous red robot that grapples the ends of the two drill pipes to be connected, and then uses a powerful hydraulic torque wrench to mate the pipes together to a precise torque level. By the time we were ready to begin coring the first sediments, the crew had put together a string of more than 2,000 meters of drill pipe, weighing more than 200 tons.

To appreciate this feat fully, rescale the ship in your mind to the size of a small Lego piece about two centimeters long. The drill string to the seafloor would appear (and behave) like a long strand of wet spaghetti more than two meters in length. But even in severe winds and swelling seas, drilling and science can continue unabated on the *Resolution* as the thrusters swivel and churn to maintain the ship's position within meters of the actual position of the borehole.

When the drill string is set, a hydraulic piston coring device is lowered down inside the drill pipe. Once this device seats itself at the bottom of the drill pipe and locks into position, the drilling operator pressures up the now-sealed drill stem with seawater. The built-up hydraulic pressure shears three finger-sized shearpins inside the coring device, and the released pressure powers the core barrel ten meters into the sediments in just under two seconds. Sediments cored in this way are recovered with almost surgical precision. The core barrel is then retrieved up and out of the drill pipe, cut up into smaller 1.5-meter sections, split in half lengthwise, and then brought to the scientists in the labs. After each coring stroke the drill pipe is advanced downward ten meters and the next core is taken. The coring is almost too efficient, swamping scientists with full ten-meter cores coming up every fifteen minutes or so.

Inside the shipboard laboratories, scientists conducted their full spectrum of analyses to date and characterize the composition of the sediments. As the first data emerged we were beginning to piece together the paleoclimatic story these sediments were telling us. Visually, the sediments appeared as a succession of alternating light and dark layers

repeating in one-meter cycles like a barber's pole: The creamy buff-white layers contained abundant carbonate microfossils, and the darker greenish-brown layers contained fine wind-blown silt-size quartz grains and clay.

As we cored deeper and deeper into the past, we began to see clear patterns in the sediments. The very regular alternations of darker, dust-rich and lighter, dust-poor layers told us that the climate of this region had experienced very regular oscillations of drier and wetter climates throughout the past many millions of years. Then we observed a long-term trend to these oscillations: The dark, dust-rich layers were both thicker and darker toward the upper, younger part of the section. They tended to get thinner and lighter as we examined the deeper and older parts of the drilled sequence.

These preliminary findings hinted at discoveries that were revealed after more than a year's lab work. Subsequent analyses on these sediments—and on comparably aged deep-sea sediments drilled off West Africa—showed that subtropical Africa became markedly drier after about 2.8 million years ago, with further shifts to even more arid conditions near 1.7 million and 1 million years ago. The thicker, darker layers toward the upper part of the drilled sequence represented times when Africa was much drier than it was before 2.8 million years ago. The amount of dust in these layers was between three and five times greater than present values. These dust layers also contained much larger quantities of microscopic glassy, dumbbell-shaped particles called phytoliths, which are structures found in savanna grasses to help them stand tall. Their relative abundance in these dust layers indicated periods when tree and bush vegetation was killed off, setting the stage for a great expansion of savanna grasslands that thrive in drier conditions. This conclusion is also supported by evidence in these same dust layers of fossil pollen from desert and semi-desert vegetation.

This "drying up" of Africa did not occur in a single, permanent step, but rather the sequence of light-dark sediment cycles shows that it happened in a series of periodic and gradually increasing pulses of aridity between 3.0 million and 2.6 million years ago. The collective weight of paleoclimatic

evidence gleaned from deep-sea sediments off West and East Africa documented the onset of much drier conditions in subtropical Africa beginning about 2.8 million years ago.

Now if you asked a roomful of paleoanthropologists to describe what the human family tree looks like for the past several million years, you might get a range of opinions. But they would likely agree that something rather dramatic happened in early human evolution between about 3.0 million and 2.5 million years ago. Although the fossil record is still incomplete and very much a "work in progress," that time period marks the single most important juncture in early human evolution since the initial separation of ape and hominid lineages more than 7 million years ago.

What happened to the hominid lineage that was so dramatic? Between roughly 3 million and 4 million years ago only one species of early hominid, *Australopithecus afarensis*—better known as "Lucy"—is known to have roamed the then-verdant African landscape. These early human ancestors were very small in stature, smaller-brained and fully bipedal (although there were indications of a part-time arboreal lifestyle). By 2.9 million years ago, however, Lucy's lineage became extinct and was replaced by two entirely different kinds of hominids.

The first group is collectively termed the "robust Australopithecines" for its much heavier-boned skeleton and distinctive facial and dental characteristics, and is represented by fossil specimens of *Australopithecus aethiopicus*, *Paranthropus boisei* and *Paranthropus robustus*. These individuals had very broad, dish-like faces with flaring cheekbones, but their most distinctive evolutionary adaptation was in their teeth. Their molars were roughly twice the size of those of people today. These molars were powered by massive chewing muscles that attached not to the side of the skull as they do in modern humans, but rather to a prominent ridge at the top of their skull, which gave the robust Australopithecines tremendous power to chew coarse foods such as tubers and roots. Their mandible was roughly three times larger than that of a typical modern human of similar size. Robust australopithecines have been characterized as



efficient chewing machines, and some paleoanthropologists have interpreted that their first appearance around 2.9 million years ago reflects a specific physical adaptation to food found in more open, arid conditions.

By about 2.6 million years ago the first representatives of our genus, *Homo*, appeared—*Homo habilis* and *Homo rudolfensis*. They were distinctive in that they sported much larger brains than any previous hominid and lacked the thickly boned skeleton and large chewing molars that characterized the robust Australopithecines. Instead, they were slighter, larger-headed individuals.

The first solid evidence of early behavioral change is detected also at about 2.6 million years ago. The first stone tools of any kind were found in sediment layers also containing *Homo* and Australopithecine remains. These tools mainly consisted of crude choppers, and evidence for their use has been found in the form of scarred cutting marks left in fossil limb bones of ancient antelope.

130 | Fossil evidence from African antelope species, documented by Elisabeth Vrba of Yale, also showed that major evolutionary changes between 3 million and 2.5 million years ago were not restricted to the hominids. Vrba documented a dramatic clustering of extinctions and the appearances of new species around 2.6 million years ago. These included the telltale first appearances at this time of species well-adapted to dry conditions, such as the oryx, whose long muzzle allows it to recondense moisture from its breath to conserve water.

Coincidence alone can't prove causality, but the match between the climate record documented by the ocean sediments and species changes documented by the fossil record is so striking that a number of researchers have recast the "Savanna Hypothesis" to suggest that a major ecological shift to drier, more open vegetation in Africa created the conditions for old species to die out or adapt and for new species to emerge.

To assess this hypothesis, we have used new methods, using volcanic ash layers to link sedimentary sequences on land and in the deep sea. We can fix the chronology of human evolution by precisely dating numerous volcanic ash layers found in ancient lake margins and river sediments that

contain the fossils. Like wind-blown dust, volcanic ash ejected from explosive eruptions of Ethiopian and Kenyan volcanoes is also carried out to sea, and we can find these same ash layers in the same deep-sea sediments that also contain the African paleoclimate record. Using electron microprobe analyses, we determine the chemical composition of the ash in the deep-sea sediments and then match these elemental "fingerprints" with ash found at the hominid sites on land. The method provides a new opportunity to test the linkages between the changes in African climate and evolutionary changes in the species living in Africa. It was these ash layers that were responsible for my excursion to Lake Turkana with Frank Brown in 1993, and ultimately for my first bout with the two o'clock meanies.

In the course of my ongoing literature searches for published research on African paleoclimate, I came across a paper that made me stop in my tracks. It was a proposal, written sometime in 1954, for an expedition to explore the deep seafloor of the Mediterranean. "The program is unique in that it will coordinate the research on the sea floor and in the sea with studies on the shores and inland," the author of the proposal wrote. "Since many deep sea cores contain undisturbed records of the complete sequence of Pleistocene events, their study is certain to yield a more complete picture of the marine Pleistocene record than it has been possible to gain through morphological studies of the abundant but incomplete sections occurring on land areas."

The proposal went on to say that "the relationships between Pleistocene climate as recorded in the sea, the occurrence of Early Man and the character of the paleolithic cultures are closely related." Among the questions the expedition proposed answering were: "What kind of climatic changes were there in prehistoric and historic times? What was the impact of the climatic fluctuations on the evolution of Man?"

These were precisely the questions I explored more than three decades after this visionary proposal, titled "The Deep Sea and Early Man," was written—by Maurice Ewing.



*"So for many years we lived a sort of  
wilderness existence,  
which perhaps is only fitting for a tree-ring lab"*





# TREE-RING CIRCUS

## *Using Trees to Reveal Earth's Environmental History*

by Gordon C. Jacoby

**Z**ealously seeking any clues that could teach them something new about the Earth, Lamont scientists would leave no stone unturned. It was inevitable that one day the observatory would look up from the Earth and into the trees.

Throughout most of the world's temperate zones and certain tropical regions, trees form one growth ring each year. The size, density, anatomy and chemistry of each ring reflects the environmental conditions in the year in which it grew. So like ancient scribes, long-lived trees can sensitively record the environmental history of a given time and place.

Silent though they are, the trees, it turns out, can speak volumes. They can chronicle temperatures, year by year, hundreds of years into the past, long before thermometers and other meteorological instruments were set out and in places where humans seldom traveled or kept records. The trees' long, precise record of annual changes can help us figure out if recent global warming has been caused by humans and help us assess the ecological effects of such warming. They can even leave telltale signs of where and when great earthquakes occurred, when glaciers advanced or retreated, when volcanoes erupted and when forest fires raged.

The study of tree rings, or dendrochronology, is a relatively new field. It was pioneered at the University of Arizona, which was the main (and almost only) tree-ring laboratory in existence when I earned my Ph.D. in geology from Columbia in 1971. I had focused my research there

on how the geology of regions affected the water supplies that flowed within. As a hydrologist in the early 1970s, I participated in a large-scale study to assess the impacts of major projects to dam, divert and redistribute water from the mighty Colorado River in the southwestern United States. Instruments had recorded a recent history of the region's renewable water supply, but a much longer-term perspective was essential. I learned that trees, whose growth depends on water, might be able to provide some hydrological history. So I began working with Charles Stockton at the Arizona tree-ring lab—looking over his shoulder, taking tree core samples, examining specimens and learning about tree-ring analysis.

We put together a 400-year record from the trees, which showed that the shorter-term recorded data did not provide a full understanding of the renewable water supply. Our tree-ring data showed that the available supply was likely to be significantly less than the recorded data suggested. In fact, our study indicated that the proposed water development and consumption could lead to hydrological bankruptcy. Many people didn't want to hear that there wasn't enough water for all the desired projects, but in the face of concerns about limited water supplies and other environmental considerations, the pace of some of the proposed developments has slowed.

By this time, I had become fascinated by the potential of trees as a tool for unraveling natural environmental history. In one of my visits to the tree-ring lab in Tucson,



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I learned that my old alma mater was proposing to enter the field of dendrochronology and to establish its own lab. But no one at Lamont knew very much about the field, so its proposal to the National Science Foundation had some obvious flaws. This was opportunity not only knocking, but breaking and entering.

I called Wally Broecker, the principal investigator of the NSF proposal, and volunteered to rewrite it with a more solid dendrochronological foundation. Broecker invited me to Lamont to give its first tree-ring seminar and to visit NSF to help introduce and promote the project to scientists there. When NSF funded Lamont's proposal, I was offered the job of launching Lamont's tree-ring lab, along with a promise of having our own building. I immediately hired Ed Cook, a bonafide dendrochronologist trained at Tucson, and the seed was sown.

The building we had been promised turned out to be a ramshackle, somewhat smelly former private house on property adjacent to the original Lamont estate. It did have a scientific history, however. The former owner was a physician who apparently had used the place to conduct experiments on mice in an effort to create smokeless cigarettes.

That wasn't the only smoke that had been blown about the place. Rooms in the house had also been promised to other Lamont researchers. In typical Lamont tradition, the building had been divvied up by researchers hungry to find any available space. But the tree-ring lab soon established the alpha position and we overtook the whole house. Mind you, our quarters—in terms of location and condition—did not exactly induce jealousy among our colleagues, and because it was off-campus and off the beaten track, we didn't get many visitors. So for many years we lived a sort of wilderness existence, which perhaps is only fitting for a tree-ring lab.

It was a sapling of a tree-ring lab, but Lamont proved fertile soil. Like most everyone else, Lamonters did not know much about dendrochronology. But unlike at some other places, I just couldn't get away with anything at Lamont. Scientists here would challenge nearly every statement I made, no matter how simplistic or fundamental

it seemed to me. I could always expect someone to say, "Well, how do you know that?" and I had to explain why a particular procedure or concept was valid. And that was very healthy, because it made me think a lot more and sharpened my understanding of the science.

At the same time, Lamonters have always been open to taking some chances and trying new applications to a technology or a science. And that suited me well because I was keenly interested in exploring whatever the tree rings could reveal about the natural environment.

As a general rule, a wide tree ring indicates that a tree thrived that year because it had warm temperatures and/or adequate water. Narrow rings indicate that growth was inhibited by cold temperatures and/or drought.

To get these tree-ring records, we core the trees, extracting a pencil-thin cross section extending from the newest rings at the bark to the oldest rings at the center of the tree. The process does not cause permanent injury to the trees. Not all trees are suitable because they don't live long enough to take us sufficiently far back in time, or because they don't produce usable annual growth rings.

We routinely sample ten to forty trees from the same site to derive the communal response of the trees' ring widths in a given location. We sand and polish the cross-sections to bring out the rings' detailed anatomy. Then by means of pattern matching and statistical analyses we assign correct years to each ring, a process called cross-dating. On live trees the outermost ring represents the year we took the sample. Preserved dead trees can be cross-dated if living trees nearby are old enough to provide overlap. For older trees we establish approximate dates with radiocarbon dating and use cross-dating to establish exact relative dates between samples.

Multiple sampling also allows us to cross-date the rings of many trees to ensure that our dating is accurate, and to compensate for occasionally false or missing rings. And just as in humans, trees' growth rates change over their lifetimes, so we have to filter out the effects of aging on the tree rings. It is not a trivial endeavor, but we have developed statistical analyses that we can run on multiple samples from the same site.

As a additional check, we compare what our tree rings indicate about temperatures or water supplies, for example, with actual records of these environmental conditions taken by thermometers, rain gauges and other meteorological instruments stationed in the region over the past twenty to roo years. If the trees and the instruments tell a similar story we can more confidently estimate the meteorological record back to times long before instruments were installed.

One of our major achievements has been to use trees to reconstruct a record of annual temperatures across the entire Northern Hemisphere over the past 400 years. We have taken advantage of trees at high-latitude tree line and some northern elevational tree lines. These trees are at the limit of their ability to survive, and small changes in temperatures stress them and leave a distinct and measurable signal in their rings. To collect our Northern Hemisphere temperature records, we have sampled boreal tree line forests of Canada, Alaska, Mongolia, Norway, Siberia and Kamchatka.

Essential for getting a long, untarnished record is finding trees that have never been disturbed by humans. Getting to our sampling sites typically requires hiking, kayaking rivers or using small planes. During field seasons, we are itinerant and live out of tents—not unlike a traveling circus. Our research expeditions are nothing if not memorable, as Rosanne D'Arrigo, the lab's first graduate student and now a colleague, will tell you in an accompanying chapter. To sample climatically sensitive hemlocks in the Himalayas in Nepal, Ed Cook and Paul Krusic have trekked to some relict stands that were thirteen days' walking from the nearest trail head—though even these trees were endangered by unregulated logging, fire and other natural hazards.

Some of the data from this network of trees we have sampled has combined to create an emerging picture of Earth's temperature over the past centuries. It provides a long-term perspective to help scientists determine what factors may be driving the higher temperatures of recent decades and whether global warming may be related to human activity, or may just be part of natural climate variation. By comparing

the tree rings with other long-term evidence, scientists will better understand whether the buildup of industrial greenhouse gases in the atmosphere is causing the recent warming trend, or whether other factors, such as solar or volcanic activity, play critical roles in Earth's climate.

The general trend reflected in the tree-ring record includes cooler conditions in the early 1700s, followed by warming that started mid-century. An abrupt cooling occurred in the late 1700s and early 1800s, varying for different geographical areas. The coldest period was between the 1830s and the 1870s, after which a steadily increasing warming trend began. Temperatures in the 1900s have been higher than in any other period captured by the tree rings from all over the Northern Hemisphere. The ten highest growth intervals are all after 1920. The highest twenty-five-year growth period was between 1944 and 1968, with additional warming in the last two decades.

Scientists are already using the emerging tree-ring records of annual temperature changes to search for the critical factors—working cumulatively or counteractively—that affect Earth's climate. In general, warmer temperatures recorded by the trees correspond with periods of increased solar irradiance before the Industrial Revolution. But solar irradiance alone cannot account for the steadily rising and unusually high warming that began in the late 1800s when large amounts of industrial greenhouse gases began to build up in the atmosphere. Volcanoes and aerosols also play a role by sending up particles that block sunlight and cool temperatures—the way the eruption of Mount Pinatubo in the Philippines did in the early 1990s. The cold trend in late 1700s and early 1800s coincides with several major volcanic eruptions.

Our research also provides insights on how ecosystems may respond to global warming—a subject of interest to forestry management officials. Many people have speculated that warmer temperatures would spur growth in the great forest belts stretching across Alaska, Canada and Siberia. But a study I did with D'Arrigo showed that such thinking might be too simplistic and that global warming



may produce complex side effects that cause ecosystems to respond in unanticipated ways.

Analyzing Alaskan tree rings dating back to the 1680s, we found that starting in the 1930s, temperatures became unusually warmer and tree growth increased. But since the 1970s, that growth has declined. Continuing warming conditions may be increasing evaporation and slowing tree growth by making trees more prone to moisture stress. Warm temperatures may also be promoting increases in insect populations and diseases.

Meanwhile, Ed Cook has been pursuing a similar tree-ring temperature record for the Southern Hemisphere, which poses more challenges for dendrochronologists. The Southern Hemisphere has far less land, with little of it located in temperate (rather than tropical) zones in which trees have annual growing seasons that produce the best annual growth rings.

But Ed Cook found a species of tree, the huon pine, that grows at high elevations near the windswept timberline of Mount Read in the outback of western Tasmania. Not only do the huon pines record reliable and detailed signals of temperatures fluctuations, they produce a natural pesticide, methyl eusinol, that also makes them highly resistant to rot. Sampling live and dead trees (some burned by forest fires but still usable for tree-ring analysis), he has reconstructed an annual temperature record reaching back 4,000 years.

The record shows an abrupt temperature rise since 1965. It also reveals above-average warmth from A.D. 1,100 to 1,190, which coincides with what is known as the Medieval Warm Epoch, a well-documented era of warmer temperatures in some regions of the Northern Hemisphere. But huon pine records also reveal a period even warmer than now, which occurred in pre-Industrial Revolution times 1,500 years ago.

Cook also discovered huon pines that were buried by sediments, and thus preserved, around the Stanley River in Tasmania. Some of these trees are up to 10,000 years old, giving Cook an unparalleled opportunity to extract an annual temperature record extending to times when the Earth was emerging from its last ice age.

Cook, D'Arrigo and Brendan Buckley are extending dendrochronological research in the Southern Hemisphere, sampling in New Zealand and southern South America. And D'Arrigo, working with colleagues in Thailand and Indonesia, is exploring whether some tropical trees such as teak might be new candidates for tree-ring research. They might be used to reveal records of past monsoons and El Niños, which regulate rainfall in the region. Back in the Northern Hemisphere, Cook has also put together a tree-ring record of drought across the continental United States, which nicely chronicles the Dust Bowl era, among other periods.

But water and temperatures are not the only possible factors that can affect trees' growth. We discovered that other traumatic events could also severely diminish trees' ability to grow and would also leave telltale marks in the rings: earthquakes, landslides, surging glaciers or volcanoes that spewed out sulfurous gases that blocked out sunlight during a growing season.

In the 1980s, working with Paul Sheppard and Kerry Sieh of CalTech, we sampled nine conifer trees growing within twenty meters of a segment of the San Andreas Fault near the town of Wrightwood, California. In all nine, we found significantly suppressed tree rings—all starting between the growing seasons of 1812-1813. In all of these trees, something happened that winter that affected their growth more than anything else during their entire life spans. It took four trees more than half a century to recover.

But farther from the fault, trees of the same species and in the same environment showed no similarly abnormal rings. We believe the root systems of the nine trees, growing on the surface rupture of the fault, were severed by the effects of a major earthquake. In several cases the trees' tops were broken off by quake-related accelerations and displacement.

The trees provided the first evidence that a major earthquake took place at that time and place. Such information is more than just of historical interest: It extends farther into the past the limited track record of that segment of the San Andreas, giving seismologists a better understanding of

how long it takes for strain to accumulate along the fault and how often major earthquakes may occur on it.

In 1992, we uncovered evidence for another great earthquake in a locale that had not been considered susceptible to major earthquakes until recently. Sonar surveys of Lake Washington, within the city limits of Seattle, had revealed that large landslides had occurred in the past, carrying Douglas fir trees to the lake bottom. The trees were preserved in the oxygen-poor sediments, and a private logging company had sought to make some money by salvaging the great trees with barge cranes. We cut a deal with the company and sampled seven trees recovered by the barge. The trees were so well-preserved, they still retained some bark and the last annual ring that they had grown before they were drowned in the lake.

We cross-dated the trees with samples taken of our submerged trees by scuba divers. The cross-dating was aided by matching fire scars, because Douglas firs' unusually thick bark can survive repeated fires that can cause nonfatal scarring or distinct signs of trauma in subsequent rings. All seven samples died in the same year about 1,000 years ago (estimated by radiocarbon analysis). Each had a completely intact outer ring and there was no indication that the next year's growth had started. So all the trees must have died in the fall, winter or early spring.

We then analyzed a bark-bearing Douglas fir log that had been partially buried on a tidal marsh about ten miles northwest of Lake Washington. The log was buried by sand deposited by a tsunami originating from Puget Sound, according to Brian Atwater of the United States Geological Survey. The tidal wave also caused the marsh to subside by three feet and sediments rushed in to partially bury and preserve the log.

Radiocarbon dating could estimate only that the log was between 1,000 and 1,300 years old and that the sand around the log was deposited between 1,100 and 1,000 years ago. But our tree-ring analysis revealed that the log and the seven submerged Lake Washington trees all died in the same season of the same year. The landslide and the tsunami were triggered by the same great prehistoric

earthquake. The tree-ring results made an important contribution to the documenting of this event, one of the major findings showing that the Pacific Northwest is a region of potential seismic hazard.

There are more arboreal detective stories to tell. Sampling trees across all of North America, we showed the pervasive effects of a great volcanic eruption in Tambora, Indonesia, in 1815-16, which was so cold that colonists on the Eastern Seaboard called it "The Year with No Summer." We found that volcanic gases had probably blocked sunlight and cooled temperatures over an unexpectedly extensive area, though there were substantial geographical variations and some regions experienced little effect from the eruption.

Our former colleague Gregory Wiles, now at The College of Wooster in Ohio, has chronicled the advance and retreat of the Columbia Glacier (and by extension climate changes) in Alaska by dating trees that had been overrun and killed during advances, and exposed again during retreats. We have found evidence of past forest fires and insect infestation. We have even tracked down the date and stand of trees from which a famous covered bridge over the Housatonic River at Cornwall, Connecticut, was built in Colonial times.

And there will be more stories that the trees will tell us. In 1995, we were invited to leave our isolation ward and enter a fully renovated on-campus building that had been the observatory's original machine shop. Our new state-of-the-art laboratory has microscopes with computerized measuring machines, based on our own design, that can measure rings to a precision of 0.001 millimeters. We have an X-ray densitometry system for analyzing seasonal and annual density variation in growth rings. We use Lamont mass spectrometers for chemical analysis of wood and plant material. We have microtomes and precision saws for sample preparation and a climate-controlled storage facility to archive our vast collection of wood samples from around the world. Lamont's new interest in terrestrial ecology has spawned new applications for tree-ring analysis.

Dendrochronology at Lamont has taken root and is branching out.





# ON THE THELON RIVER

## *A Dendrochronological Expedition*

by Rosanne D'Arrigo

I was the first graduate student in Lamont's Tree-Ring Lab (TRL), and it was the end of my first year. I had been on a few field trips, but this would be my first to the real back country. With Gordon Jacoby, the lab's leader, and Jobie Carlisle, a research technician, I would sample trees in the Thelon River Sanctuary in the Northwest Territories (NWT) of Canada in the summer of 1984. This is a true wilderness area in the great north woods, about midway between Yellowknife, NWT, and Churchill, Manitoba.

The Thelon Sanctuary encompasses the northern latitudinal tree line, where trees are literally living on the edge. For these trees, small changes in temperature can mean the difference between thriving and just hanging on until next year. The trees' fortunes are reflected clearly in their annual growth rings, and thus, the rings serve quite reliably as thermometers that can record annual temperatures hundreds of years into the past.

Taken between sites previously sampled in Coppermine, NWT and Churchill, the collection of samples in the Thelon River Sanctuary would fill a sizable gap in our network of sites for northern North America. We would eventually use these and other data to reconstruct annual temperatures for the Arctic and Northern Hemisphere dating back to the 1600s.

The area we were targeting was at the junction of the Thelon and Hanbury Rivers, a full three-hour flight by Beaver or Twin Otter charter plane from the town of Yellowknife, the nearest point of civilization. Our pilot

landed on the river near a sandy bank. During the scramble to get our waterproof bags and other gear onto land, I managed to drop Gordon's shotgun (our only defense against bears except for a few mothballs and a plastic horn) into the river.

It was like being dropped onto our own private planet. Just the three of us and a few gazillion mosquitoes to remind us that we were only humble visitors. Not being too proud, I grabbed one of the mosquito headnets, removing it only in the tent, between quick bites at mealtime or on the occasional windy day. We traveled downriver in our three (mostly) trusty inflatable boats, stopping to sample any trees that caught our eye along the way.

We carried our traditional camping fare in our duffel bags (including the somewhat tedious tuna, macaroni and cheese casserole that Gordon insisted on) and supplemented it by catching grayling and Arctic char, which were so abundant that even I, a novice, could get some. Once Gordon had proudly laid out his catch on a rock to clean for dinner, and an aggressive seagull (is there any other kind?) swooped down and grabbed it. Jobie and I stifled our laughter. I got my comeuppance when my one stash of coffee (I was the only true addict of the group) went overboard. I watched in helpless horror as the grains dispersed. I insist I was only mildly irritable, for a few days at most.

The possibility of a meaningful bear encounter was never far from my thoughts. The dunk in the river notwithstanding, Gordon's shotgun was operational and he insisted that I have a target practice session "just in case."



LAMONT-DOHERTY EARTH OBSERVATORY:  
TWELVE PERSPECTIVES ON THE FIRST FIFTY YEARS

My aim being what it was, I fervently hoped that I could rely on the mothballs when (or if) the time came. But I never lost the urge to look over my shoulder. It wasn't until years later that Gordon told me of the extensive bear scarring on the trees around one of our campsites, which had escaped my notice; ignorance can be bliss.

One night we decided to really go native. We had brought along a book of local lore that described a recipe for "Indian ice cream." Its main ingredient is the fruit of the soapberry bush, so called because it contains saponin, a soaplike chemical that causes the berries to lather when crushed. The recipe called for whisking the berries (Gordon handily fashioned a whisk from a nearby branch) to a frothy pink pulp. The final instruction was to "sugar to taste." We started to add a few teaspoons, which did not make the slightest dent in the bitter soapberry flavor. Finally in desperation we dumped in our entire sugar stash for the rest of the two-week trip. Haagen-Dazs it was definitely not. From then on we played it safe, picking only the scrumptious cloudberry and blueberry (the bears like these too) that abounded in the area.

We did have some other sweets during the trip. Gordon's fiftieth birthday passed on the river, and to mark the occasion, Jobie had lugged a mail-order gourmet chocolate whiskey cake, which along with some Bailey's Irish Cream in our tea more than made up for the soapberries.

We could keep track of years easily enough, but hours soon became more difficult. In the middle of the wilderness, particularly in the north where the light is almost constant, it is nice to have at least one mainstay of civilization, the trusty watch to plan one's day. We each had one at the start of the trip, but in quick succession, Jobie's and then Gordon's stopped running. It was all up to me and Gordon said, "Whatever you do, don't lose your watch."

The next day we sampled some very old spruce trees surrounding the ghostly ruins of a cabin in which a British adventurer named John Hornby starved to death in the winter of 1926-27. Having a bit of a death wish, he had purposely waited for the caribou migration to pass so that

overwintering would be a bit more of a challenge. He and his hapless nephew, dragged along for the ride, both perished. Gordon called that it was time to go, and we were well downriver before I realized that I had dropped my watch somewhere near John Hornby's grave.

At the end of the day we beached the rafts, watchless. Jobie and I began to make camp and get a fire started. Gordon had plunked himself down on the beach and would not be budged. He had made up his mind to fix his watch, no matter what. He ignored our calls for dinner and would not look up until he had managed to take the thing apart and get it ticking again. It was a masterful feat, although the watch died again the next day. I can't help thinking that it was meant to be that we would lose this last crutch of civilization in this timeless place. I recalled reading that John Hornby's watch had stopped around the time of his death that fateful winter. We never again missed knowing exactly what time it was.

All too soon it was time to get picked up by our pilot at our prearranged rendezvous area. There is always that element of uncertainty that the pilot might forget about us or be off on a bender somewhere, and not having a radio we bantered uneasily about our chances of getting out on our own—virtually nil. But at last he did show up and we took off into the air, disturbing a herd of muskoxen on the way. About midway into the monotonous three-hour journey I happened to glance over at the pilot. He was sound asleep.

Fifteen years have now passed, and we are thinking that we simply must return to the Thelon River for resampling to update the Hornby tree-ring chronology. It would be interesting and useful to see how the trees have been faring over this recent period. Are the warmer temperatures spurring more growth? Or is the warming increasing evapotranspiration in the area, creating drier conditions that stress the trees and stunt their growth? The answers would tell us a lot about how the forests may react to possible greenhouse warming and related environmental change.

Next time we will bring a good deal more sugar for the soapberries.



*"Our goal was not to predict an El Niño,  
but to understand it."*





# THE HEAT IS ON

## *Creating the First Computer Models to Predict El Niño*

by Mark A. Cane

On the Fourth of July weekend of 1985, it first dawned on me that we were ready to try forecasting El Niño.

I was reading a scientific paper at a lake my family had visited every summer since I was seven years old. The paper's authors claimed to foresee El Niños in heat variations in the northwestern tropical Pacific Ocean. I wasn't convinced, and I didn't understand what they had done to squeeze a portentous signal out of the data. But one message did come through: It's the heat. The key to El Niño was not its shifting winds or sea surface temperatures or atmospheric pressures. The starring role belonged to the fluctuating heat within the vast reservoir of the tropical Pacific.

The following Monday, back at Lamont, we went to work. Some years earlier, Steve Zebiak, then my graduate student, and I had aimed to create the first computer model capable of simulating an El Niño—essentially translating into mathematical equations all the complex physics involved in the processes that spawn the phenomenon. Our goal was not to predict an El Niño, but to understand it.

Not that we were uninterested in prediction. The desire to have foreknowledge of climate variations must be as old as agriculture, and as widespread. Maybe more so, since even hunter-gatherers would have had good reason to wonder about the weather that lay ahead. In our culture the most ancient and famous climate forecaster is the

biblical Joseph, who interpreted Pharaoh's dream of seven fat and lean cows in terms of years of fertility and famine.

Also fresh in our minds was the 1982-83 El Niño, the most disastrous in at least a century, which had taken everyone by surprise. In September of 1982, a meeting of experts had reached a consensus that no El Niño was forming, though an extreme one was already well under way. In 1982-83, equatorial waters from the South American coast to the dateline warmed by an average of 2°C, with warming along the coast exceeding 6°C. Prevailing westward-blowing trade winds actually reversed directions. The climatic consequences were devastating. In Australia the worst drought ever recorded sparked firestorms that incinerated whole towns. Normally arid regions of Peru and Ecuador were inundated by as much as three meters of rain. California beaches were rearranged by unusual winter storms. Drastic changes in the tropical Pacific Ocean resulted in mass mortality of fish and bird life. All in all, the 1982-83 El Niño event was responsible for an estimated \$8 billion in damages and the loss of 2,000 lives.

El Niño has been documented as far back as 1726. Originally, the term referred only to a warming of the coastal waters off Ecuador and Peru that always arrived around Christmas, the celebration of El Niño, the Christ child. In most years the warming was mild and benign, but then, as now, occasionally severe warmings led to widespread mortality of fish and guano birds, crippling



the local economy. Heavy accompanying rainfall spawned catastrophic flooding in coastal areas.

We now know that this coastal warming is just one oceanic component of a dramatic global climate cycle known as El Niño and the Southern Oscillation, or ENSO. When an El Niño occurs, warm surface waters that are usually pooled in the western Pacific expand dramatically throughout the tropical Pacific Ocean, from the dateline to the South American coast, until warm water girds a quarter of the Earth's circumference. Rain clouds that accompany the warm waters migrate eastward, taking rain from places where it is expected and dropping it unexpectedly in others. Prevailing trade winds diminish, rearranging global atmospheric circulation patterns and therefore weather patterns throughout the globe. On average, an El Niño occurs about every four years, but the cycle is highly irregular. Sometimes there are only two years between events, sometimes almost a decade. And the intensity of the events varies considerably.

I first became interested in El Niño after the terrible winter of 1977, when I was still at MIT. It seemed that the persistent weather pattern that brought storms to the Northeast was somehow related to the ongoing El Niño in the tropical Pacific. I had a background in mathematics, meteorology and tropical oceanography, and I was less interested in how the bad weather was generated than in why the El Niño was there in the first place. Prediction was even further from my mind.

The culture of modern science rejects auguries—so much so that “understanding” is often regarded as a higher goal than prediction. But I was free of that particular prejudice because my Ph.D. advisor, Jule Charney, who contributed as much as anyone to our understanding of how the atmosphere works, also was responsible for the very first weather forecast based on a numerical model. Of course, in the ways of modern science, prediction follows from understanding, not from reading the entrails of pigeons. And in the earth sciences, understanding always begins with the observational facts—which makes

it salutary for a theoretician like me to be in an observatory like Lamont-Doherty.

I arrived at Lamont in 1985, more than a decade after Doc Ewing's death. But his spirit still dominated the place, and still does, it often seems. Although scars from old battles that I couldn't begin to understand were occasionally exhibited to me, sometimes proudly, sometimes bitterly, his positive legacy was a far stronger presence.

Doc established Lamont as a place to collect data in order to understand the Earth. I didn't quite fit Doc's mold. Not only had I not grown up at Lamont, I didn't collect data, I didn't go to sea, and I didn't step into a lab. I wrote equations on yellow pads, fed instructions into computers, and waited to see what the calculations had to say. Still, I like to think that Lamont saved me from writing equations about other equations, and instead got me to write equations that paid attention to data. I know it made a difference to be in a place that recognizes no limits and is always ready to engage a wider world. Lamonters are intellectually fearless: No problem is beyond reach, even if (perhaps *especially* if) the conventional view puts it outside the boundaries of one's narrow specialty. All in all, Lamont provided the right conditions for trying to tackle a complicated, far-ranging phenomenon like ENSO.

When we began our quest, much was already known about ENSO. We built on the work of others. The atmospheric component of ENSO, the Southern Oscillation, had been discovered by Sir Gilbert Walker, the director-general of observatories in India. Walker assumed his post in 1901, shortly after a historic famine resulting from the failure of monsoon rains in 1899 (which not incidentally was an El Niño year). Gilbert set out to predict monsoon fluctuations, an activity begun by his predecessors after the disastrous monsoon of 1877 (also an El Niño year).

Walker was smart enough to realize that whatever was affecting the Indian monsoon was larger than India. Sorting through world weather records, he became aware of work indicating that over a period of several years, the average barometric air pressure at sea level in the Pacific

seesawed up and down across a region ranging from South America to the Indian-Australian region. Over the next thirty years he gathered an array of observations from people and stations all over the globe. For example, he found that when the barometric pressure was low in Tahiti in the central Pacific and high in Darwin, Australia, in the eastern Pacific, there was heavy rain in the central equatorial Pacific, drought in India, warm winters in southwestern Canada and a cold ones in the southeastern United States. But Walker's findings were mostly dismissed—probably because his conclusion relied on observations made over a relatively short time span, and because he didn't come up with a conceptual framework based on physics to support the patterns he found. But re-examined with subsequent decades of new, independent data, his global correlations have been confirmed.

Oddly, despite his thoroughness, Walker failed to make the connection between the Southern Oscillation and El Niño. It was not until the 1960s that Jacob Bjerknes not only pointed out the relationship, but also proposed an explanation for it. His ideas evolved from observations of the atmosphere and the tropical Pacific Ocean gathered during 1957-58, the International Geophysical Year.

A major El Niño occurred in those years, bringing with it all the atmospheric changes connected with Walker's Southern Oscillation. Bjerknes thought it was implausible that a warming confined to coastal waters off South America could cause global changes in the atmosphere, and the 1957 data showed that the rise in sea surface temperatures was not confined to the coast. Bjerknes suggested that the South American coastal events known locally as El Niño were just part of a much more extensive warming trend. He was correct, and the term "El Niño" is now most often used to denote changes that span the entire tropical Pacific.

Bjerknes noticed that in non-El Niño years, sea surface temperatures in the eastern end of the Pacific are remarkably cold for such a sun-drenched equatorial region. Temperatures of surface waters—and the air above them—

contrasted sharply with those in the very warm western Pacific. In a natural process to equilibrate this contrast, cooler eastern Pacific air flows along the sea surface toward the warm west Pacific: Those are the trade winds. In the west, the air is supplied with moisture from the readily evaporating warm water, generating rainfall. There it is heated and rises, leaving the surface with less air pressure, and so drawing in cooler (and denser) air from the east. Bjerknes called this equatorial circulation system—generated by the temperature and air pressure gradients between the eastern and western Pacific—the Walker Circulation. He thought that fluctuations in this circulation in the tropical Pacific coincided with pulses in Walker's Southern Oscillation, which caused atmospheric circulation changes throughout the globe.

At the same time, Bjerknes said, the prevailing winds drove warm tropical Pacific waters westward, as well as northward and southward toward the Earth's two poles. To replace those departing waters, deeper (and colder) waters upwell in the eastern Pacific. The cold waters reinforce the east-west temperature gradient, keeping the winds blowing westward and warm waters pooled in the west. Bjerknes noted that the interaction could operate in the opposite direction. If the trade winds diminished, that would allow warm surface waters to migrate eastward, reducing the east-west temperature gradient, which would further diminish the trade winds—a chain reaction.

Thus, Bjerknes married the circulation of the ocean and the atmosphere—El Niño and the Southern Oscillation—together in a continuous feedback loop. He offered a persuasive hypothesis for both the normal and El Niño states, but he stopped short of explaining the ENSO cycle, the perpetual shifting from warm state to cold state and back again.

In the 1970s, the oceanographer Klaus Wyrtki introduced an essential additional factor—the movement of tropical Pacific waters. Knowing that the data didn't exist to discern what the Pacific was doing, Wyrtki attacked this problem in a manner Ewing surely would have



endorsed. He set up a network of tide gauges on Pacific islands to record monthly average sea levels. His measurements showed that sea levels were lower in the eastern Pacific and higher in the west. But in the months immediately preceding the El Niño of 1975-76, sea levels began to rise in the east. Warm waters were actually being transferred from west to east. -

All of this was known in the early 1980s, but it was not at all clear which of these things were essential to the genesis of an El Niño event, and which were incidental. Other observations that once seemed important turned out later to be red herrings. According to one theory at the time, wind changes at the South American coast were the prime ENSO mover, but more data showed that, in fact, the coastal winds didn't change during an El Niño. Another theory started things off with wind changes in the South Pacific east of Australia. In this case the observational facts survive with more and better data, but the theory does not.

146 | From the beginning of our work on El Niño, Steve Zebiak and I conceived of our model as an embodiment of the essential physics outlined by Bjerknes and Wyrski. Well, not quite from the beginning: There were a goodly number of false starts—as usual in science. Our numerical ENSO model depicted in a simplified manner the evolution of the tropical Pacific Ocean and overlying atmosphere. When we ran the model in our computer, it generated recurring ENSOs at irregular intervals. Most significantly, we didn't have to throw in a volcanic eruption, solar variations, biological activity or anything else to set the cycle in motion or keep it going. The cycle resulted solely from interactions between the ocean and atmosphere.

Our model is a dynamical rather than a statistical one; that is, it is built from the governing physical equations rather than from a sequence of past observations. A great virtue of dynamical models is that once you have gotten them to simulate something accurately, you can then go back and examine the physics behind whatever they have simulated.

By analyzing our model, we and others developed a now widely accepted theory to explain why ENSO perpetually recurs. Think of the tropical Pacific as a huge bathtub, with water sloshing back and forth. At first, warm water is piled higher in the west than the east, making the surface seem like a seesaw riding higher in the west. But if we could look down into the depths, we would see that the thermocline—the thin line separating the lighter, sun-warmed waters of the upper ocean from the colder, heavier waters of the abyssal ocean—is tilted oppositely: up in the east, down in the west. The thicker layer of warm water between surface and thermocline in the western Pacific packs more heat.

The westward-blowing trade winds push the waters up in the west. But the winds and the waters are not in continuous synchrony, so warm western waters eventually slosh back to the east, raising the temperatures of upwelling eastern waters. El Niño is launched and Bjerknes' positive feedback takes over: The temperature gradient between west and east weakens, the trade winds slacken, and even more warm waters rush eastward. With more warm water now in the east, surface water temperatures warm more, the winds weaken more, and on and on. El Niño is strong now; the thermocline starts tilting downward in the eastern Pacific, where the greater heat content now resides.

This is where Bjerknes got stuck. How does the system get out of the El Niño, and flip back to a cold state? The answer lies in the continual imbalance between the relatively fast movements of winds and waters at the surface of the Pacific and the more powerful but more sluggish movements of the great volume of water between surface and thermocline. The winds and the heat content are out of sync. In the long haul, the heat content signal moves through the depths, uninfluenced by circumstances occurring at the surface, at its own slow but strong pace, driving the system into the next ENSO phase.

If this dynamic was right, we could forecast El Niño by keeping track of the varying depths of the thermocline in

the tropical Pacific. The deeper the thermocline, the more heat the ocean contains. The heat was the key. But on that Monday after the Fourth of July in 1985, we faced a significant obstacle: No means existed to collect the data we needed most, the thermocline depths.

We worked around this by using data we *did* have—observations of surface winds collected by merchant ships. The model launched the simulated ocean by using the wind data to create currents, thermocline depths and ocean temperatures. By starting with different wind data, we created different initial conditions, each of which were the model's best guess at the actual conditions at a particular time in the recent past. Then we ran the model to see how the combined ocean-atmosphere system would cause each of these initial sets of conditions to evolve. We made "predictions" based on real past conditions and then compared the predictions directly with what actually happened.

We began with July 1982, the time when the largest El Niño of the century was already well under way and no one knew it. We wanted to give our first try at forecasting the best possible chance. This first forecast was a success: The model results evolved into an amplified warming that peaked at the end of the year. It is the only thing I have done in my scientific career that worked the first time, and it worked despite some bugs in the computer code. Mercifully, it worked even after the bugs were fixed. We then went back in time, starting forecasts from April 1982, then January 1982, and so on. It kept working: With the exception of April 1981, all the forecasts from October 1980 on were successful.

Over the next weeks we went on to try forecasts at three-month intervals for the entire period from 1970 to mid-1985. We couldn't go earlier because the wind data coverage before 1970 was too spotty. And, of course, we couldn't go later. The results clearly demonstrated that the model had predictive skill at lead times out to at least one year. In the early fall we wrote up the results and sent the paper off to the journal *Science*.

It was rejected. One reviewer argued that *Science* shouldn't publish our paper because its severe length limits on articles made it impossible for us to provide enough details on such important research to satisfy a cautious reader. But the broader reason for the rejection was a widespread and not unreasonable skepticism that El Niño could be predicted at all.

Many people still believed that each El Niño was a separate event triggered by some sequence of random, unpredictable changes in tropical weather. Our idea that ENSO was the systematic evolution of a cycle held little currency in 1985. With his analogy that a flapping butterfly wing could launch an unpredictable sequence of events, Ed Lorenz had convincingly demonstrated that the chaotic nature of the atmosphere ensured that weather could not be predicted more than two weeks ahead. So, how could we claim to predict anything in the climate system a full year ahead?

The answer has two parts. First, the tropics are more predictable than temperate latitudes. Second, we were not trying to predict weather at a particular place and time, but average conditions over a large area and over several months. Nothing known in 1985 precluded the possibility of doing so. On the other hand, nothing guaranteed that it could be done, and the claim to have done it came out of the blue, with little prior work to make it plausible.

Meanwhile, we began to notice that all the forecasts we initiated in 1985 agreed in calling for an El Niño event of moderate size at the end of 1986. Our retrospective forecasts from Januarys had been especially skillful, so when the January 1986 forecast continued to predict an El Niño, we began to consider what to do. Should we keep it within the academic community? After all, it could be wrong. The 1982-83 El Niño was a still a vivid, unhappy memory in Peru, Australia, California and other hard-hit places. Warning that another was on its way, even if it was expected to be much smaller, would surely cause alarm. But it could be right—in fact, our research said it most likely was right.



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Shouldn't we warn people? If so, how? We had no experience with the press.

Realizing that going public would inevitably put Lamont into the public eye, I came in one Saturday in late winter to lay out the case to Barry Raleigh, Lamont's director, and Paul Richards, a highly respected seismologist whose work on nuclear test detection had given him experience dealing with the media on complicated scientific issues. I remain grateful for their strong support and sound guidance. (Perhaps it was also fortunate that Barry Raleigh, who had spent much of his career at U.S. Geological Survey on earthquake prediction, was so taken by the idea that *anything* could be predicted.) In their judgment, the science was sound, so it was proper to go forward with it. We shortly sent off a report to the journal *Nature*. We also made arrangements for a press conference and a news release that helped reporters understand a science that was then brand-new.

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By and large we were pleased with the way the story was reported. But publicizing the forecast did not receive universal approval from our colleagues in oceanography and meteorology. Far from it. One of the more positive comments was: "I thought it was great. Either you would be right, in which case it would look like we all knew what we were doing, or you would be wrong, in which case we could have a good laugh at your expense." In addition to the usual reluctance of most scientists to do anything applied or to have anything to do with the public, the very idea that climate could be predicted a year ahead was a radical notion.

The eastern equatorial Pacific warmed through the first few months of 1986, in keeping with an oncoming El Niño. But as if it had just been waiting for us to say something, the ocean began to cool shortly after our March press conference, and it stayed cold through the summer. It was a tense time for us: We had stuck our necks out pretty far, and it looked like we would pay for it. The August data brought us some relief: The ocean finally began to warm. Nature caught up with the forecast during the

fall, evolving rapidly into a moderate, though unmistakable, El Niño. We counted the forecast as a success, although differences in timing and other details showed that the prediction scheme was far from perfect.

I am sure we made the right call in taking our forecast to the public in 1986. It was too new and untested for anyone with sense to rely on it without corroborating evidence, but it served as a wake-up call to pay close attention to the state of the Pacific, with beneficial results in Peru and elsewhere. Our successful forecast demonstrated that ENSO forecasting was for real. It added momentum to the nascent international Tropical Ocean Global Atmosphere (TOGA) research program, which had just been launched a year earlier with a goal of trying to predict ENSO. Over the next decade, the TOGA program would establish a string of monitoring buoys spanning 10,000 miles of the equatorial Pacific, as well as other observational systems. These gave ENSO researchers essential real-time data (including the thermocline depths that we did not have when we started) to use in their computer models and to explore the ENSO phenomenon further.

The idea of forecasting gained more acceptance in 1991 when many people saw "unmistakable" signs of an El Niño brewing and our model predicted (correctly) that an El Niño wouldn't come. The El Niño arrived in 1992, as our model predicted.

We had planted the seed that ENSO predictions could be used by society to lessen ENSO's impact on agriculture, water resources, energy, health, fisheries, tourism and so on. But making forecasts and disseminating the information worldwide to those who could benefit from it was a larger and more complex task than our small research group could handle. In 1989 I had begun making the case to committees of the National Academy of Science and to our National Weather Service that we needed a center dedicated to forecasts of the ocean-atmosphere system and climate over several months—something akin to what the NWS does for the atmosphere and weather over several days.

The following year, an international group of scientists, under the auspices of the National Oceanic and Atmospheric Administration (NOAA), drafted a proposal to create an international research institute for climate prediction. By then our original concept had evolved considerably. The institute would encompass an end-to-end system, beginning with improving forecasts of climate variations such as ENSO; then going a step further by predicting impacts on agriculture, health and economics; and finally completing the mission by designing and implementing public policies and strategies to respond to predicted climate changes. Anticipating an El Niño, for example, policymakers might urge farmers to plant water-tolerant rice instead of cotton in Peru, water managers to conserve water in Australia, public health officers to ensure medical supplies for possible climate-related epidemics such as cholera, or commodities traders not to export surplus corn in southern Africa—just as Pharaoh decreed the building of granaries and stockpiling of grain during the seven “fat” years.

In 1993 I attended a workshop on ENSO, famine and early warning systems in Africa. (Naturally enough, the meeting was held in Budapest.) Roger Buckland, who was with the Southern African Development Community, showed a graph of twenty years of annual maize yields for Zimbabwe. I instantly recognized that the graph was almost identical to one I was quite familiar with—a graph of equatorial Pacific sea surface temperatures. The picture in front of us showed that the maize yields in Zimbabwe, almost half a world away, follow the ENSO cycle. So I said that we could predict the Zimbabwe maize yield well before the October-November planting time.

The reaction of disbelief was immediate, strong and, except among the other climate scientists in the room, unanimous. The agronomists and other knowledgeable people were quick to point out that only my ignorance of how maize is grown could account for my simplistic view. They noted that maize crops would be greatly influenced by social factors, such as political shifts and the size of farms, the introduction of new hybrids, and whether or not

rain fell at precisely the right time (during inflorescence). The reading I have done since convinces me that all their doubts were well-founded. (It also left me with the feeling that it's a miracle that even a single ear of maize ever makes it to harvest.) Nonetheless, the facts are the facts. Not only is maize yield related to ENSO, but the relation is stronger than the relation of rainfall or any other obvious climate variable. Apparently the maize plants integrate the rainfall, cloudiness and temperature effects of ENSO into a signal stronger than any of the component parts.

The appearance of a paper co-authored by Buckland and me, “Forecasting Zimbabwean maize yield using eastern equatorial Pacific sea surface temperature,” caused a considerable stir, publicizing the idea that ENSO forecasts could affect human affairs. It came only two years after the worst drought in southern Africa this century, which adversely affected 100 million people. The 1991-92 El Niño was predicted well in advance, so a drought should have been anticipated. But it was not, leading to a costly famine relief effort.

We've made truly remarkable strides since 1982-83, when climate experts never saw a momentous El Niño coming even as it was on the way. Today, many research groups are doing routine ENSO prediction using a variety of methods. Regular observational updates for the tropical Pacific and summaries of forecast results are available at a number of Web sites. The 1997 El Niño brought a high level of awareness to the public around the world. In a clear sign of progress, regional Forecast Forums were organized in 1997 in southern Africa, at which local experts and decision makers got together with international experts to assess the ongoing El Niño and its likely impacts on the region.

While the first ENSO forecasts elicited widespread surprise that it could be done at all, now the usual question is why the forecasts aren't better. Doubts about applying ENSO forecasting are justified. The forecasts are not perfect. Our forecast failed dismally in 1997, which turned out to be one of the largest events of all time.



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As Casey Stengel put it: "I had many years that I was not so successful as a ballplayer, as it is a game of skill." At first we thought the failure might have resulted from something new in the present state of the climate system, something our rather simple model cannot handle. However, subsequent experiments showed that the model would have been able to predict successfully if we had fed it with better initial data.

The relationships between ENSO and its climate impacts are not straightforward. Even if the ENSO forecasts *were* perfect, in most cases we can't honestly predict a sure outcome, but only a shift in the odds—for example, a higher likelihood of drought in southern Africa over a season or year, or fewer hurricanes in the Atlantic. The 1992 drought in southern Africa was the most severe in at least a century, but the El Niño that year was only a moderate one. And while the correlation between ENSO and climate impacts is relatively strong and reliable in the tropical Pacific region and contiguous continents, it is less certain beyond the tropics, where the climate system is more chaotic and thus less predictable.

In these more far-flung latitudes, ENSO events don't lead directly to droughts or flooding; rather, they introduce a bias that tends to shift the climate system from its usual patterns. El Niño events increase the likelihood of heavy rains in the Great Basin region of the United States, and cold (La Niña) events raise the chances of Midwestern drought and lower corn yields (as in 1988). Certain atmospheric patterns are more likely to persist, altering the paths of hurricanes, typhoons and winter storms.

There is real information here, but can one use it to the benefit of human society? Figuring out how to factor it into decision making is a complex, multidisciplinary problem.

In 1996 NOAA selected a consortium of scientists from Lamont and the Scripps Institution of Oceanography to lead a newly created International Research Institute for Seasonal to Interannual Climate Prediction. The IRI is a focal point for global efforts to capitalize on our progress in climate prediction and to make it responsive

to the needs of all nations affected by climate variations. It fits right into Lamont's long tradition, first encouraged by Ewing, of building global, multi-institutional, multidisciplinary research programs.







## AFTERWORD

### *The Lay of the Land: A Personal Recollection*

by Tom Christopher

As I learned soon after coming to work at Lamont-Doherty in 1977, the observatory was not just a workplace. It was home.

The Lamont grounds owe much of their unique character to the observatory personnel. The scientific and support staff all felt a personal involvement with the place.

Sometimes, this could be annoying. Manik Talwani, the observatory's director, had hired me to rescue Lamont's neglected grounds, and one of the first projects I undertook as Lamont horticulturist was to excavate the lily pond in the formal garden, which had been filled with sand a decade previously. Emptying the concrete-lined pond was pick-and-shovel work, and I and some teen-age summer workers spent days wheeling that sand out, wheelbarrow-load by wheelbarrow-load. As we did, one elderly member of the clerical staff made a point of warning my employees that she was sure that they would find buried there explosives left over from old seismic surveys. Another woman returned several times to entertain us with apocryphal stories of toddlers who had drowned in that pool. But when the job was done and the pool filled with water, John Sindt and the Lamont machinists turned a load of cast-iron scrap scavenged from a New York City demolition site into the most elegant gates imaginable, so that no one should have to worry about the security of their children.

Everyone contributed according to his or her talents. Art Garrick, Building & Grounds' carpenter, proved to be a virtuoso glass cutter, and turned a truckload of discarded storm windows into the panes with which he and I reglazed

the Lamont greenhouse. For several years that little glass house turned out thousands of seedlings every spring to fill Lamont flower beds.

On one occasion, when I was removing a large dead elm from the lawn between Lamont Hall and the director's house, I discovered that some long-ago tree surgeon had filled the trunk's hollow core with concrete. Though I could fell the trunk, I couldn't cut it into sections, so that I was left with a log fully three feet in diameter and ten feet long. Even the Buildings & Grounds backhoe couldn't lift that.

Then Chick Defelice, the mechanic who maintained B&G's machinery, remarked that as a boy, he had worked at logging in the local woods with nothing more than a buck saw and one old horse. Climbing aboard the backhoe, Chick tipped up first one side of the log and then the other, gradually walking it up into the back of a dump truck.

The place belonged to all of us, and we all took care of it and shared it. In the fall, when the orchard in front of the Lamont mansion filled with apples, Lamonters of all stripes gathered some to make pies or cider. In the 1950s and 1960s the original Lamont scientists and their families plowed and tilled a large field of vegetables behind the old machine shop. They shared the work and then shared the harvest. In later years that flat field became a fertile site of spirited soccer games that brought together scientists, technicians and students hailing from many continents.

The Lamont grounds had a scientific lineage that predated Maurice Ewing. Anyone entering Lamont from



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Snedden's Landing has seen the inscription "Torrey Cliff" on the old gate. This was the name that Thomas Lamont gave to his estate, and it honors an earlier resident. John Torrey, born in New York City in 1796, was a chemist and botanist who played a key role in turning the study of natural history in the United States from a gentleman's hobby to a professional discipline.

Torrey was, in fact, the most renowned American botanist of his day, a mentor to Asa Gray and his collaborator in the preparation of the first two volumes of the *Flora of North America*. As a professor at Columbia's College of Physicians and Surgeons in New York, and at Princeton, and as a trustee of Columbia College, he was equally important as a scientific educator. From 1854 through the end of the Civil War, he summered in a farmhouse set on the cliff atop the Palisades. It was located near the former site of the Lamont director's house, which was recently torn down to create Lamont's new international climate research center. Torrey's house burned even before Thomas Lamont acquired the property and built his estate. But while I served as horticulturist at Lamont, it was still possible to find in the woods the remains of John Torrey's garden.

A generation ago, older residents of Palisades still preserved stories of John Torrey's botanizing expeditions through the surrounding area. When he died in 1873, Torrey bequeathed his personal herbarium of 40,000 specimens and his scientific library to Columbia University. In 1899, Columbia turned these over to the newly founded New York Botanical Garden, where they served as a foundation block for what have grown into the premier botanical research collections in the world. So although the earth sciences may be just half a century old at Lamont, a review of the landscape's history reveals that the spirit of scientific inquiry on this site is much older.

I remember exactly when I discovered the Lamont landscape. Or, to be precise, when I discovered what that landscape was supposed to be. I had spent the best part of a morning badgering Dick Greco at Buildings & Grounds about the location of a drain. My plan (one that Dick had

no intention of following) was to find the original drain from the pond in the rose garden, so that someone (not me, naturally, but someone like Dick who understood those things) could clear it. Then, I explained to Dick, I could let the water out of the pond and clean out its accumulated muck without the use of a dredge. Eventually, Dick ran out of patience and, handing me a drainage plan for the whole of the campus center, he threw me out of his office.

When I sat down to examine Dick's gift, I found that it was almost unreadable, a copy of a copy of a blackline architectural plan. It didn't show any drain from the pool. But in a bottom corner of the sheet, I found a date, 1929, and the ghost of an inscription, one that credited the drawing to Olmsted Brothers of Brookline, Mass.

Even as a newly graduated horticulturist, I knew what that meant. Olmsted, in the person of Frederick Law Olmsted and the firm he had founded in 1858, had designed most of this nation's great park systems, from Central Park to Yosemite. I knew that he and his successors at the firm had also designed private estates for the greatest of the robber barons. And maybe, it now seemed, for Thomas Lamont, as well. It was as if, while dusting some neglected oil painting, I had discovered the signature "Rembrandt."

I'm embarrassed to admit it, but at the time I could think of nothing better to do than call directory information. I got on the telephone and dialed up an operator in eastern Massachusetts, and asked her if there was any listing for an Olmsted in Brookline. She put me through to Olmsted Associates. When I called that number, a very efficient female voice informed me that yes, they were successors to the firm of Olmsted Brothers, and that yes, of course they still had a file on the Lamont estate. What sort of office did I think she was operating, her impatient tone of voice implied. My guess is that woman still had, filed under "P," the receipted bills for the pencils Frederick senior had used to work out planting plans for Central Park.

At any rate, in a letter of 26 October, 1978, I was informed that Olmsted Associates had in "our vault" 187 sheets of plans of the Lamont estate, and that these included

"general studies, topographic mapping, grading plans, profiles and sections, planting plans and architectural/site details." Though signed by one Artemas P. Richardson, I have no doubt who the real author of that letter was.

Her firm hand also controlled the billing. When, after inspecting the index she sent me, I requested copies of thirteen plans, I was told that copying them would cost exactly \$305. Talwani was nearly always supportive of my efforts, but he was taken aback by my request for this sum. Perhaps on this occasion he was calculating how many maps Marie Tharp could print out for the price of those Olmsted plans. But Talwani's deputy director, Ellen Herron, came to my rescue, as she often did. A serious gardener herself, she was as curious as I was to see what was on those old blueprints.

When, several months later, they arrived, they revealed a landscape remarkably similar in spirit to Lamont Hall. Designed in the local Dutch Colonial style, Lamont Hall is unusual because it is so simple; though built on a grand scale, it is not grandiose. Similarly, the landscape Olmsted Brothers had envisioned, was breathtaking in its sweep, but designed around elements that Thomas Lamont must have remembered from his boyhood in a Hudson River Valley parsonage. For example, in a memo of October 15, 1930, the designers specified the planting of some 27,500 bulbs of crocus, narcissus and scilla into the lawn south of the house.

Yet this stupendous display was not laid out in the formal arrangement of geometric beds typical of the period. Instead, these spring flowers were to be scattered in bunches throughout the grass underneath apple trees, much as you would find daffodils springing up in some old upstate orchard. Lamont had hired Olmsted Brothers to design his estate in 1927, but according to the planting plans, the bulk of the installation came in 1929, 1930 and 1931. Apparently, the stock market crash had little effect on the resources that Lamont, as a banker at J. P. Morgan & Co., could command, except that undoubtedly, it made labor cheaper and easier to find.

Unfortunately, little of the original planting, other than the shade trees, remained by the time of my arrival at the observatory in 1977. Only in the wall-enclosed formal garden, which Olmsted Brothers had designed as a Gertrude Jekyllesque display of perennial flowers, had any significant number of flowering plants survived. Dominating this space were several magnificently overgrown English boxwoods. According to the Olmsted records, these had been purchased in the spring of 1930 as shrubs four to six feet in diameter, and such ancient specimens must have been salvage, plants lifted from some old Tidewater plantation.

Other survivors in the walled garden were three of what had been six tree peonies, a single plant of the herbaceous peony *Festiva Maxima*, a bush of winter jasmine, and a handful of blue hostas and Virginia bluebells. A few of the plants in this garden had not only survived, but flourished. The wisteria that Olmsted Brothers had trained over the arbors at the garden's periphery had vaulted out and up into adjacent trees, while in what had been the perennial beds, masses of spiderwort continued to spread aggressively despite all my attacks with spade and fork.

What I didn't find in the formal garden or anywhere else on the observatory grounds, were the roses called for in the Olmsted plans. These had exciting but unfamiliar names, such as *Emily Gray*, *Silver Moon* and *Ile de France*. And though I never found those particular bushes, I did identify a couple of other rose survivors, *American Pillar* and *Dr. W. van Fleet*, turn-of-the-century antiques which surely dated from the landscape's days as a private estate.

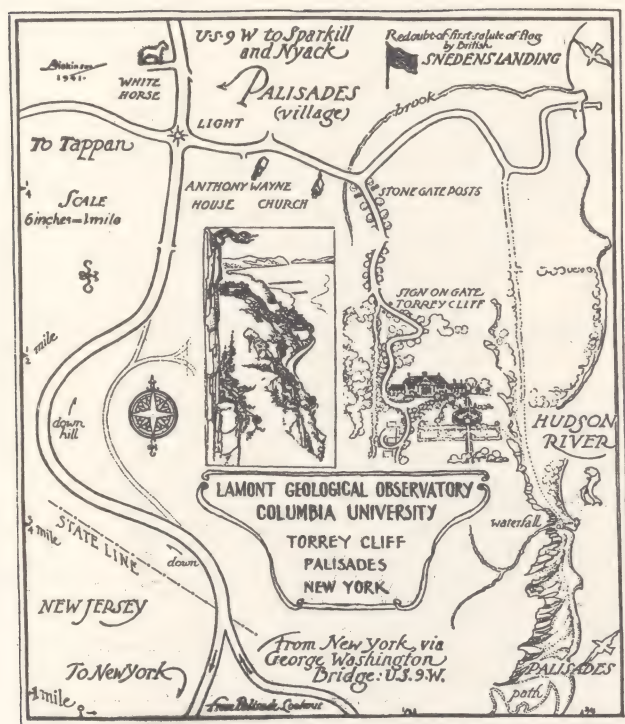
The skills I learned in this Lamont rose hunt I later took with me on travels all over the country, seeking neglected or forgotten species of cultivated roses and gathering experiences that eventually inspired a book, *In Search of Lost Roses*. My search for the Olmsted roses also inspired me to replace the vanished perennials in the formal garden with beds of antique roses. For many springs thereafter, the perfumes drifting out through the arbors were overwhelming.





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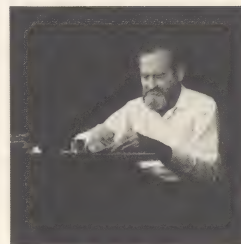




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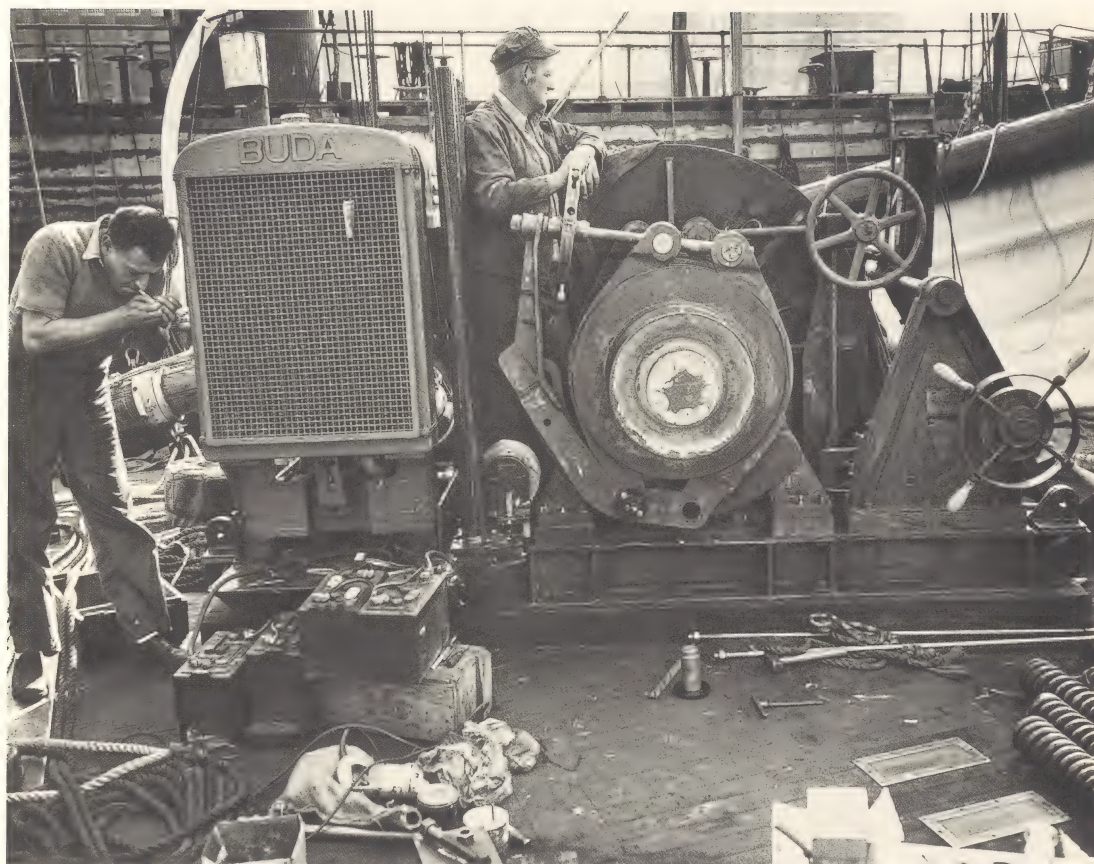




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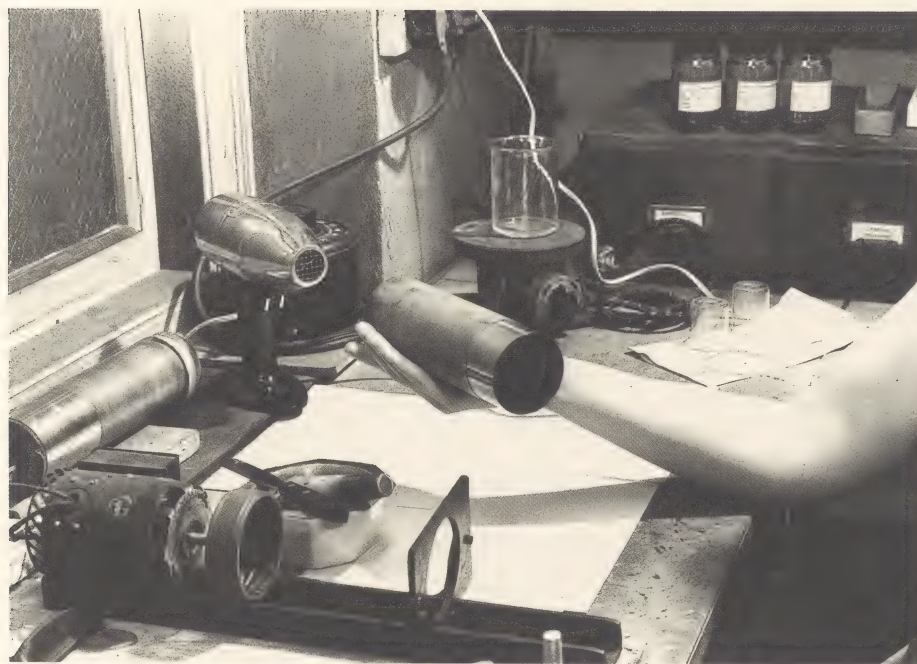




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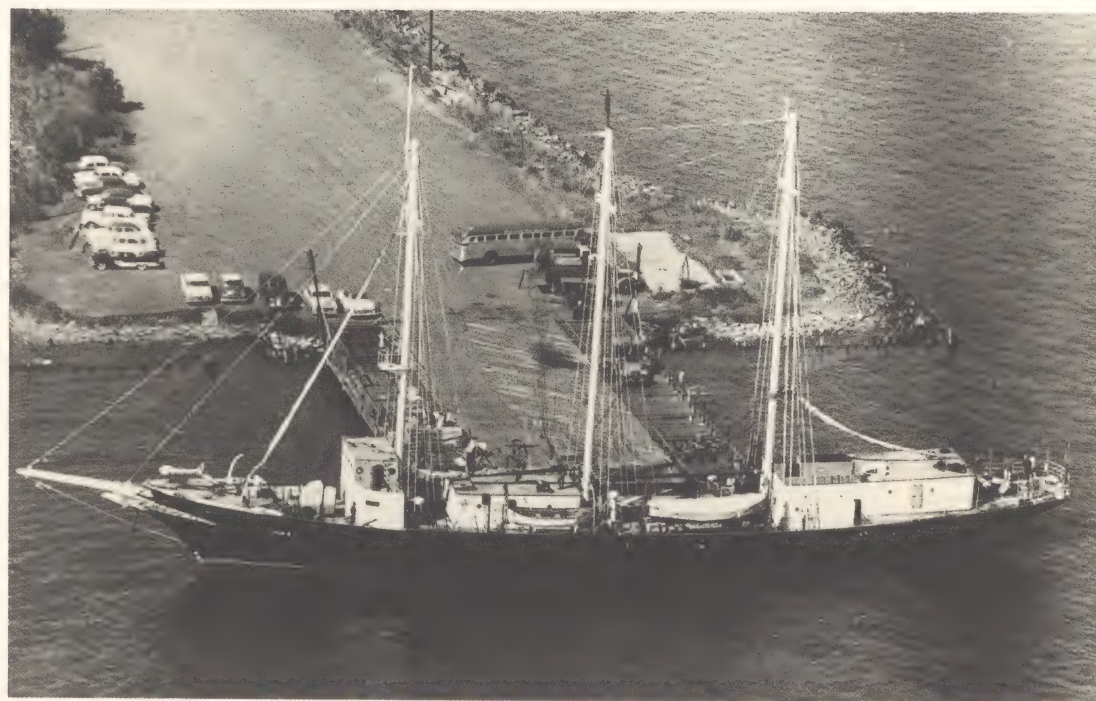


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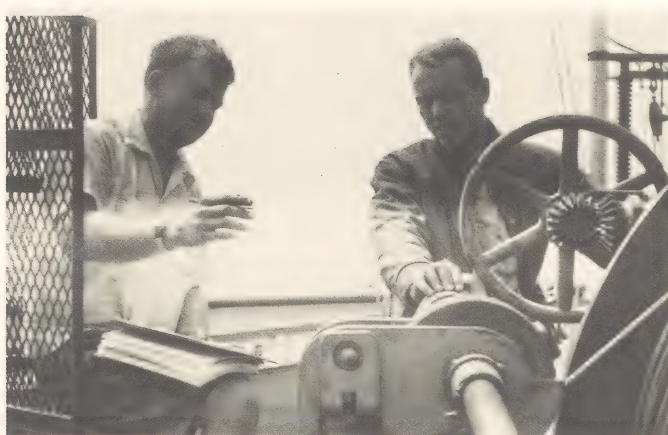


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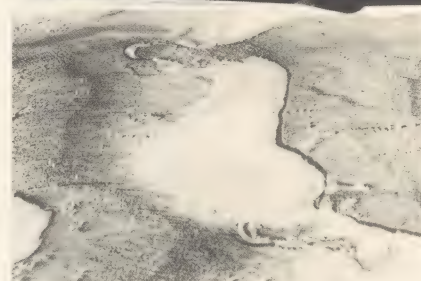




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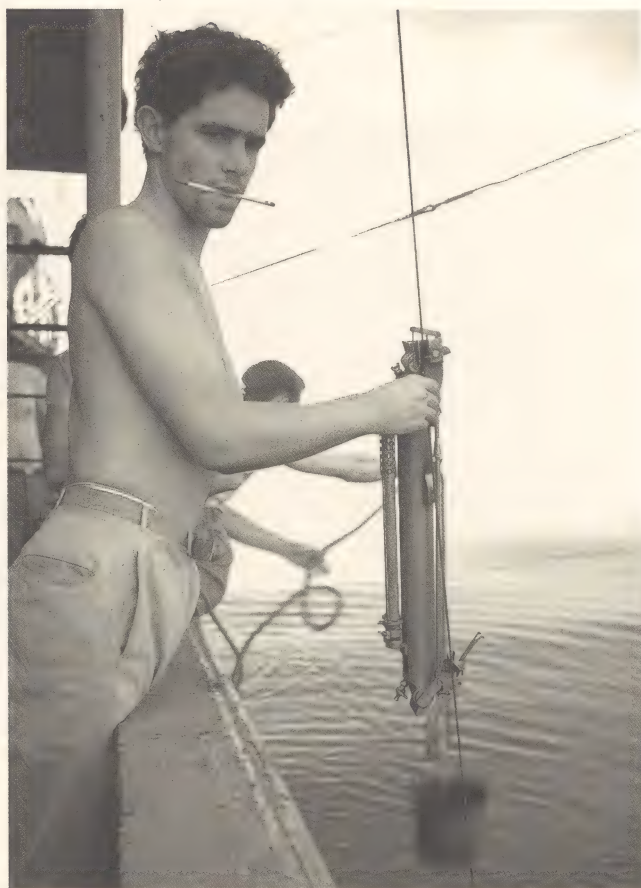


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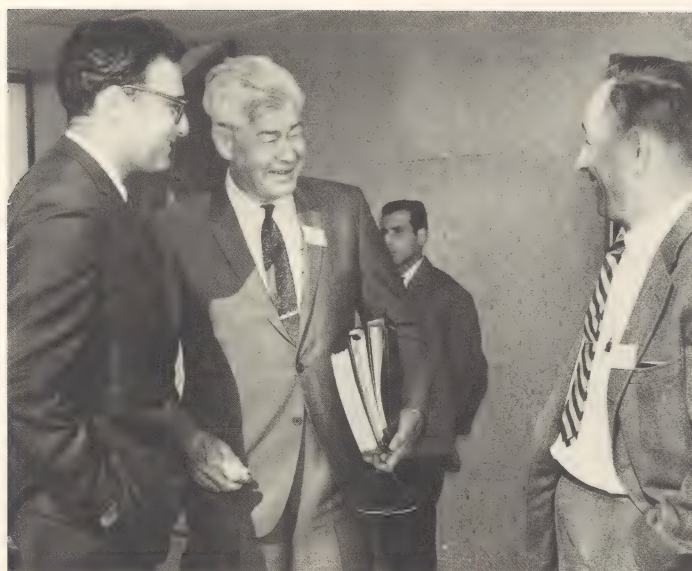




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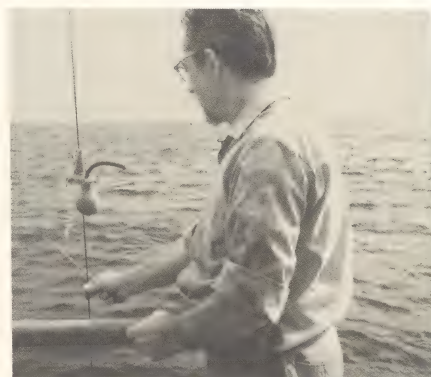
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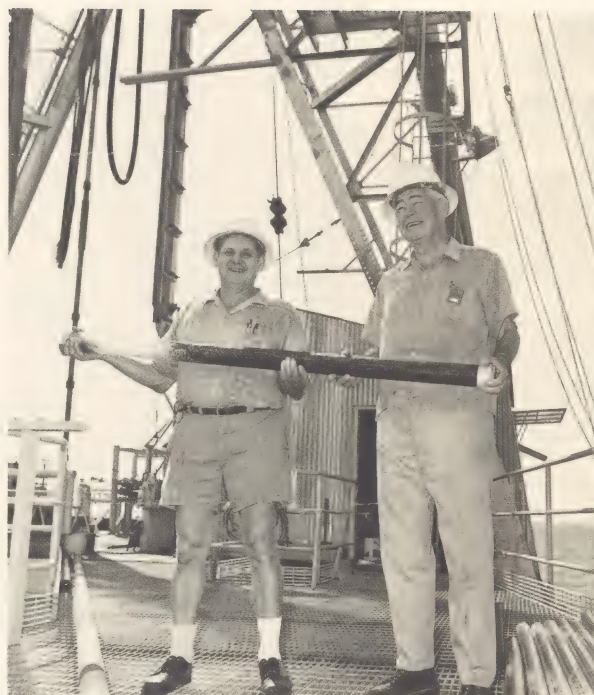


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